PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

From June 4, 1891, *to February* 25, 1892.

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

June 4, 1891.

The Annual Meeting for the Election of Fellows was held this day.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Statutes relating to the election of Fellows having been read, Sir Erasmus Ommanney and Professor Meldola were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society :—

Anderson, William.
Bower, Prof. Frederick Orpen, D.Sc.
Conroy, Sir John, Bart., M.A.
Cunningham, Prof. Daniel John, M.D.
Dawson, George Mercer, D.Sc.
Elliott, Edwin Bailey, M.A.
Frankland, Prof. Percy Faraday, B.Sc.
Gilchrist, Percy C.
Halliburton, William Dobinson, M.D.
Heaviside, Oliver.
Marr, John Edward, M.A.
Mond, Ludwig.
Shaw, William Napier, M.A.
Thompson, Professor Silvanus Phillips, D.Sc.
Tizard, Capt. Thomas Henry, R.N.

Thanks were given to the Scrutators.

June 4, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read :—

I. "Experiments on the Discharge of Leyden Jars." By OLIVER J. LODGE, F.R.S. Received May 2, 1891.

EXPERIMENTS ON THE DISCHARGE OF LEYDEN JARS.

The following experiments among others were made in the course of 1888, beginning in February of that year. A brief account of the early experiments, with some of the deductions from them, was given in a couple of lectures to the Society of Arts in March, 1888, on Lightning Conductors; and in the 'Electrician,' vols. 21, 22, 23, under the same title, a number of others were published at length, viz., the series of experiments relating to "the alternative path." But the rest of the experiments has never been published in any detail; though, as they led to some interesting observations concerning electromagnetic waves, and incidentally measured the velocity of transmission of a pulse along an isolated wire, they ought to have been written out for publication long ago.

I now venture to communicate them to the Royal Society, beginning with such brief account of the earliest experiments as may suffice to render the steps intelligible.

Description of Jars Used.

1. The pattern of jar ordinarily used was an open cylinder without lid or neck, with the charging rod firmly supported from the interior and quite free from the glass above the tinfoil.

They were of two principal sizes, which I call for short "gallon" and "pint."

Each gallon jar was 40 cm. high and 13 cm. diameter, coated to within 10·5 cm. of the top; and the capacity of the pair chiefly used was 0·0062 microfarad each. Two in series had a capacity of 28 K metres. Each pint jar was 16·5 cm. high and 8·2 cm. diameter, and was coated to within 5 cm. of the top. The capacity of the one chiefly used was 0·0016 microfarad. Two pint jars in series had a capacity of 6·6 K metres.

In addition to these ordinary jars, a couple of large condensers were made, each consisting of 16 pairs of 11-inch square tinfoil sheets, separated by double thicknesses of window glass, each pane about $\frac{1}{10}$ inch thick, and with a good margin; tinfoil strip connectors protruding on alternate sides, and copper wire prolongations, with all joints soldered, terminating in a pair of knobbed rods projecting upwards through stout glass tubes more than a foot apart; the whole thoroughly soaked and embedded in a mass of paraffin, poured molten into a strong teak outer case $22 \times 20 \times 13$ inches, the whole when finished weighing about 3 cwt.

The capacity of one of these condensers was 0·028, of the other 0·02, microfarad. Single glass thickness would have given much greater capacity, but preliminary experiments showed that single thicknesses of glass were punctured by very modest sparks.

It is important in these experiments to have joints better made than is usual for high-tension electricity. Fizzing or sparkling inside jars is abominable.

Account of the Long Conductors used in the Early Experiments.

2. Round the Lecture Theatre,* supported on four vertical posts a good way from every wall, were stretched and supported, either by silk thread or silk ribbon according to the strength demanded, four or five wires, two of them of copper, one thick (No. 1 B.W.G.) and the other thin (No. 19); two of them of iron, one thick (No. 1) and the other thin (No. 18). They are called respectively "long thick copper," "long thick iron," "long thin copper," "long thin iron." Sometimes a "thinnest iron" of No. 27 B.W.G. was used too. The thick wires formed a rude rectangle 840×515 cm.; being joined mechanically not far from their ends by a foot or so of silk ribbon, and sufficient free ends being left to connect directly with jars or machine; connexion being usually made by wrapping tinfoil tightly round the joined conductors. The thinner wires formed rather larger rectangles.

Particulars of these conductors here follow:—

	Length.	Diameter.	Ordinary resistance.	Approximate effective inductance.	Approximate capacity.
No. 1 copper	27·1 metres	0·74 cm.	0·025 ohm	390 metres	5 metres
No. 1 iron ..	27·1 ,,	0·71 ,,	0·088 ,,	390 ,,	5 ,,
No. 19 copper	30·3 ,,	0·085 ,,	2·72 ,,	570 ,,	3½ ,,
No. 18 iron..	30·3 ,,	0·12 ,,	3·55 ,,	550 ,,	3½ ,,
No 27 iron..	30·3 ,,	0·035 ,,	33·3 ,,	630 ,,	3 ,,

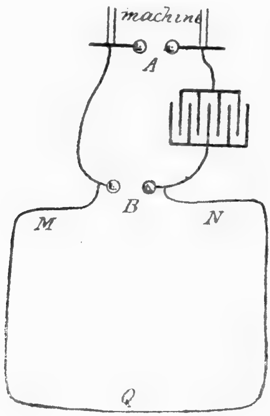
* University College, Liverpool.

The copper is commercial quality and evidently of miserable conductivity. I afterwards got some real copper from Messrs. Thos. Bolton and Sons, and with it the phenomena are still better marked.

Early Experiments.

3. The large glass condenser (0·028 mfd.) was charged through one or other of the long wires, and a choice was offered the discharge, so that it might go either round the wire or leap an air-gap, as it chose; as shown in fig. 1.

Fig. 1.



A are the ordinary terminal knobs of the Voss or Wimshurst machine where the spark occurs; B is the discharge interval acting as a shunt to the wire or other resistance. MQN represents diagrammetrically one of the wires round the room. The spark-length B was adjusted so that it was an off chance whether the discharge chose it or the wire. It was noticed that when the discharge chose B the A spark was strong, but when the discharge chose the wire the A spark was weak. The difference appeared to be only in the noise or suddenness of the spark, for when a Riess's electro-thermometer was inserted in the circuit it indicated about the same in either case.

A capillary tube was filled with very dilute acid so that its resistance was about $\frac{1}{4}$ megohm, and was connected across the B knobs instead of the long wire. When this acid tube was thus made the alternative path, and the B knobs placed so far apart that the discharge was obliged to choose it, the A spark was very weak, being reduced to a quiet spit, which could be analysed by a slowly rotating mirror into several detached sparks.

After a number of readings of spark-length, which have been elsewhere published (and which showed among other things that it made very little difference whether the alternative path were copper or iron), a common Leyden jar was substituted for the condenser, and similar results were obtained with it.

But it was now noticed, in addition, that the jar frequently overflowed by sparking over its lip; and that when this happened a spark still occurred at B though not at A.

A special overflow or short-circuiting path was then provided, equivalent to a pair of discharging tongs; calling this air-gap C, it was found that, according to the adjustment of the width of spark-gaps, flashes at B and C could be got without A; or at A and B without C; or at C only. (This was the beginning of experiments on overflow.)

Putting acid resistance into the circuit at M or at N weakens but does not stop the B sparks; and it has the same effect at M as at N. But inserting resistance at Q does not weaken the B spark perceptibly; neither does cutting the wire there; only of course, in order to permit the charging of the jar in this case, the B gap has to be bridged by some imperfect conductor; this shunt high resistance, which may be a piece of dry wood or anything just sufficient to convey the *charging* current, having no appreciable effect upon the B spark.

But it was noticed that when the wire was cut at Q a singularly long spark or strong brush discharge attempted to jump the space there whenever the machine spark occurred. (This was the beginning of experiments on "recoil-kick.")

It was also found that connecting the machine side of the jar to earth (the long wire, not interrupted anywhere, being insulated) increased the strength of the B sparks very much, and made them easier to get. Evidently the wire was acting as one coat of a condenser, the wall being the other coat. Even when the jar was discarded, no connexion being made in its place, and the wire alone used, sparks occurred at B perfectly well whenever the machine gave a spark at A. (This led to experiments on "the surging circuit.")

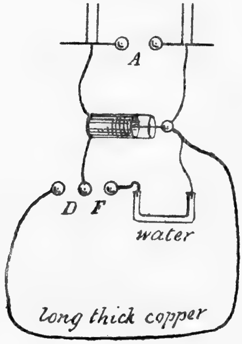
Experiments on Overflow (February, 1888).

Small Jar.

4. Tried the arrangement shown in fig. 2, the jar being pint size, as described above, of plain cylindrical shape, open at top, with its lip projecting 2 inches above the tinfoil so that the overflow distance was 4 inches. The long wire was the 30 yards of No. 1 copper. In addition to the machine spark-gap A, a couple of other intervals labelled D and F were also provided; the spark-gap D being led up to

through the long thick wire, the spark-gap F through the capillary water tube of high resistance already mentioned. The A knobs were each 2·34 cm. diameter. The size of the others does not seem to be recorded.

FIG. 2.



Separating the machine knobs too far for a spark there, sparks could be got either at C or at F or over the lip of the jar, or in two or three places at once. The lengths were D = 0·72 inch, F = 0·68 inch. Bringing the A knobs nearer together, a distance of 0·57 inch, it went there too. The A spark is the noisiest, then D, and lastly F; F is in fact quite weak. When it sparks at D it mostly goes at F too, and likewise overflows the lip of the jar, but not always.

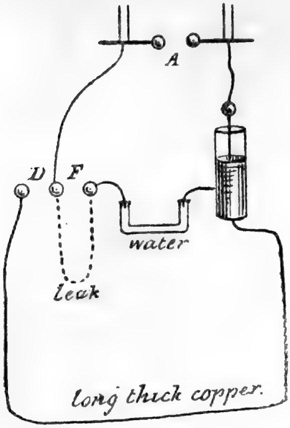
Shorten all the air-gaps so as to avoid overflow, and they spark simultaneously at the following distances :—

A.	D.	F.
0·435	0·565	0·575

Modified the plan of connexions to that shown in fig. 3; the second water resistance or "leak" being now introduced merely in order to give the jar the possibility of charging.

Whenever an A spark occurs, a considerable range is permissible with the others. As to F, it does not matter how short that is made; it is affected by the others, but has no effect on them. The overflow of jar specially accompanies a spark at D. Frequently sparks occur in all four places at once; and at times the overflows of jars are violent and numerous, so that, when A and D are both pretty long, flashes fly from cork and wood and almost anything that happens to be in contact with the jar. (The jar stood on a wooden block on an insulating stool: it was principally from this that flashes sprang sometimes.)

FIG. 3.



The following readings give an idea of the range of adjustment permissible; all the flashes in a horizontal line occurring simultaneously :—

Length of Sparks (in inches).

A.	D.	F.	Jar lip.	Remarks.
0·48	0·53	0·48	Overflowed (4 inches).	
0·48	—	0·48	Quiet.	
0·48	0·42	0·37	Overflowed.	
0·69	0·32	0·45	Overflowed.	Here F began to fail.
0·69	1·03	0·0	Overflowed violently.	Here D began to fail.
0·69	1·03	0·9	Flashing from wood or anything.	Here F began to fail again, or to be replaced by other flashes.

Thus, with a long D spark, F could be anything up to nine-tenths of an inch; whereas, with a short D spark, it failed at half that distance. The jar-overflow is precipitated by a moderate A spark if D occur too. D can be much longer than A. If both A and D are long, the overflow is violent.

Larger Jar.

Now replace the first pint jar by one of the large "gallon" jars of similar open shape, but with the glass protruding 4 inches above the coatings, so that its overflow flash was 8 inches long.

(The capacity of the jar was 0·0062 microfarad.)

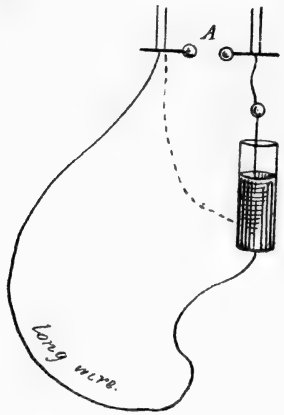
With A spark 0·62 inch long, the D and F gaps might be anything, but so long as the D spark was allowed to pass the jar overflowed every time the machine gave a spark at A.

On putting one terminal of the machine to earth (the one not attached to the jar), the D spark is considerably lengthened; and, even when the knobs are widely separated, brushes leap from each into the air whenever an A flash occurs.

Simplified Connexions.

5. Tried now this same gallon jar connected up to the machine in the simplest possible manner, either direct by a foot or so of ordinary wire, or else by the long thick copper round room or some other long wire, or sometimes by both, as shown in fig. 4, so as to see what difference the length of connecting circuit made to ease of overflow.

Fig. 4.



The machine's knobs were gradually separated until the jar flashed over its lip, and then their distance apart was read. It was found that with the long connector a very much shorter A spark was sufficient to cause overflow than with the short-circuiting wire. And not only was it shorter, it was incomparably quieter; the jar seemed to overflow without any trouble or violence when attached to the long circuit, whereas, when this was short-circuited out, the A spark had to be long to cause an overflow, and when it occurred its violence was great, as if threatening to smash the jar. If, under these circumstances, the short circuit was removed and the long wire replaced, the jar overflowed, not in one streak, but in a torrent or

cascade of sparks; the number of these splashes gradually decreasing down to one again as the spark A was shortened.

It was also found that after an overflow another was more likely, whereas after a failure another failure was probable: that there was, in fact, a kind of hysteresis, the conditions of overflow being easier for a decreasing A spark than for an increasing one of the same length. This seemed especially noticeable when the long connector was thin copper, instead of being so thick and massive as the No. 1 copper on the one hand, or so highly resisting as thin iron on the other.

The table on p. 10 summarises the readings. The full contrast does not come out strong in the early numbers: there is some caprice about whether the jar overflows or not, probably having something to do with the state of the glass surface.

The contrast comes out best towards the middle of the table. The "thick copper" and other long wires are those specified in § 2.

Spiral Conductor.

6. Another connecting path was now made, consisting of 8 yards of the No. 1 copper wound into an open spiral about a foot in diameter, and suspended in air by ribbon, as indicated by the dotted line in fig. 5; when in use, its two ends were led, one to a machine terminal, the other to outer coat of gallon jar, whose inner coat was connected to the other machine terminal.

This being so, the lengths of machine spark needed to make the jar overflow (round its lip always) under different circumstances were again read as follows:—

	Kind of connector used.	Length of a spark needed for overflow.
Gallon jar.	Thick copper spiral	0·61 inch.
	Short circuit	1·50 ,,
	Spiral again	0·63 ,,
	Long thick wire round room	0·57 ,,
	Both this and spiral in series	0·56 ,,
	The two in parallel	0·62 ,,
	The spiral alone again	0·61 ,,
Pint jar.	Thick copper spiral	0·58 to 0·52 inch.
	Thick wire round room	0·51 inch.
	Spiral	0·53 ,,
	Short circuit	1·1 ,,
	Spiral	0·54 ,,
	Thick iron wire round room	0·66 ,,
	Iron and copper round room in parallel	0·62 ,,
	Iron alone	0·67 ,,
	Copper alone	0·52 ,,
	Short circuit	1·4 ,,
	Copper again	0·52 ,,

Connector used between machine and outer coat of jar.	Length of A spark able to make jar overflow (in tenths of inch).	Remarks.
Short wire	7·0	
Long thick copper wire	5·5	
Long thin copper wire	from 6·65 to 7·4	According to which it did last.
Long thin iron wire	7·8	
Short wire again	9·5	
Long iron shunted by short wire	11·5	No overflow.
Long iron alone	11·5	Still no overflow.
Thick copper again	6·4	Overflows every time until gap is shortened to this.
Thick copper shunted by short wire	17·0	Does not overflow till this long and noisy spark is reached.
Long thick copper alone	6·2	Still overflows even at this, the spark being gentle.
Retain thick copper. Earth one knob of machine	5·25	Jar still overflows.
Retain thick wire, but earth jar end of it	5·9	
Now earth machine end of it	6·25	
Short circuit it once more	17·0	Still does not overflow.
Simple thick wire alone once more	5·6	Overflows.
Thin copper wire	min. 6·4, max. 7·1	A little indeterminate, according to whether overflow or failure happened last; that which happened last being easiest to get again.
Short circuit again	—	A has to be enormous before it overflows.
Thin iron wire	9·2	With this thin iron wire the overflow point seems definite, whereas with the thin copper it was not.
All three long wires in parallel	6·4	
Thick wire again, but with a bridge across trying to shunt out all but about 3 yards of it	6·5	
Short-circuit again added	10·3	
Remove the bridge but leave the short-circuit	from 8·7 to 10·2	No apparent reason for this shortness.
Disconnect one end of thick wire, but leave short-circuit	9·4	
Disconnect both ends, having only short-circuit	9·4	So now evidently the jar is easier to spark over, as it was at the beginning.
Restore thick wire simply	5·5	

Effect of High Resistance.

7. Interpose the capillary liquid tube ($\frac{1}{4}$ megohm) in the circuit of the thick copper wire, putting it at one or other end of it, and the jar refuses to overflow, although the spark-length A is increased to $2\frac{1}{2}$ inches.

The spark is quiet, long, and zigzaggy. The resistance has the same effect at either end, but the spark seemed straighter when the resistance was at jar end of long wire.

To test effect of putting resistance into the *middle* of a long connector, both the thick wires round room (one copper, the other iron) were joined in series and used as connector. Overflow began when A = 0·6 inch. The wires were now disconnected at their far ends, and the capillary tube made to bridge the gap. The jar now refused to overflow, though A was more than trebled in length. (Fizzing stopped it at that point.)

Contrast between C Path and Overflow.

8. But when an artificial overflow path is supplied to the coatings (as indicated by the strong line to a C knob in fig. 5) the matter is different. It does not now feel the effect of a long circuit as different from that of a short one. The space at C being 0·94 inch, a spark jumped there sometimes and sometimes at A = 0·75, with the high resistance interposed in the two long leads; and just the same happened when the resistance was removed and the long wires directly connected.

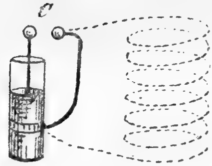
Shorten A to 0·64, and it was unable to select C, but it jumped the lip of the jar instead. It preferred 8 inches of jar-lip to 1 inch between the C knobs. When strong enough it would seem to go at C; when too weak for that it jumps the edge; but this is not a clear account of the matter. A better statement is the following:—

An A spark precipitates an overflow (*i.e.*, over the lip of the jar), but it does not precipitate a C spark. When a spark occurs at C there is quiet at A. The A and C sparks are alternative, not simultaneous. Moreover a C spark does not cause overflow. An A spark can easily occur without the edge of the jar being jumped, but the edge is never jumped without an A spark. (Connexions being as in fig. 4, with the addition of a short C or artificial overflow path, as shown by the thick line in fig. 5.)

Long Connector in C Circuit.

9. But now the thick copper spiral above mentioned (§ 6) was arranged to connect one of the C knobs with the outer coat of its jar

Fig. 5.



(as indicated by the dotted line in fig. 5; the strong-line shunt being removed), one of the two long thick wires round the room being used to connect up the machine to the same outer coat, as in fig. 4. Under these circumstances, simultaneous sparks *could* be got at A and at C, and both about the same length, but not when they are too long, say, A = 0·52, C = 0·57 inch. But now the jar can be made to overflow by either spark if of sufficient length. Thus if A = 0·61 or if C = 0·74, the jar lip gets jumped, and sometimes the A spark occurs, sometimes the C, but not both. Another reading: A = 0·69 or C = 0·94; jar overflows in either case.

Restore now the usual short wire to the C knobs, and the C spark still often goes, but it has no effect on the jar. The A spark makes the jar overflow as before.

But if the long lead between machine and jar be short-circuited-out (as by the dotted line of fig. 4), while the thick copper spiral still joins up to the C knobs (as indicated by the dotted line in fig. 5), then A cannot make the jar jump, while C can easily.

Thus overflow is always easily produced by the action of the spark occurring in a long good-conducting lead, not in a short or bad-conducting one.

Effect of Iron Core.

10. Using the thick copper spiral as before (§ 6) to make the pint jar overflow, I tried whether inserting large massive iron bars in it as a magnetic core would have any effect. There happened to be three large bars, each about 3 inches in diameter, which were used. They were of soft iron, and intended for the legs of an electromagnet.

No effect was found. The length of the A spark needed to make the jar overflow was, as near as one could tell, the same, whether the iron was in the spiral or not. Thus:—

Without iron	A = 0·53
With one bar in spiral	0·51
With three bars............	0·515

No difference that one could be sure of.

Effect of Capacity.

11. The spiral was now shunted out by a couple of Leyden jars in series, *i.e.*, with their knobs touching either end of it and with their outer coats connected. If the jars only touched one end of the wire, they had no effect; but when they touched both ends, a larger A spark was needed to cause overflow.

With the spiral alone	A = 0·53
With the capacity shunt.....	A = 0·76

Experiments on Large Condenser.

12. It was not desirable to expose the large condenser § 1 to such conditions as would make it want to overflow, because overflow with it would mean bursting; but one of the pint jars was arranged on it as a safety valve, and it was then connected up to the machine. On now taking machine spark at A, the pint jar might or might not overflow its 4 inches.

With very short connexions	A = 0·5 inch did not overflow it.
With wires each a yard or so long..	A = 0·4 inch was sufficient.
And with spiral of thick copper...	A = 0·3 inch was enough.

Iron Core Again.

13. Tried a stout spiral of brass wire (a spiral spring about a foot long and an inch diameter); it made the jar overflow fairly easily. Then inserted in the spiral a bundle of fine iron wires wrapped in paraffin paper, but could detect no difference whatever, *cf.* § 10.

Summary.

14. The noteworthy circumstance in all these experiments is the remarkable action of a long thick good conductor in causing the jar to overflow, especially if it be insulated, the most powerful conductor for this purpose being one with considerable self-induction and capacity but very little resistance. Evidently such a conductor assists the formation of an electric surging, whose accumulated momentum charges the jar momentarily up to bursting point. Resistance damps the vibrations down, and short wires have insufficient electric inertia and capacity to get them up. Iron, whether massive or subdivided, shows no effect whatever on the effective inductance of a circuit surrounding it.

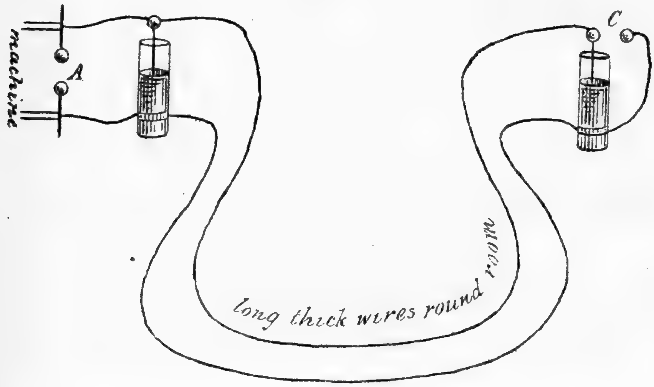
It is also noteworthy how far more readily a jar overflows directly between its coatings over the lip than it does through a pair of

discharging tongs held round the lip. Probably the sharp edges of the tinfoil contributed to this effect, possibly also dust or other specks on the surface of the glass, or it may be the action of the air film itself, but it seems as if the extremely small inductance of such a path likewise aids what, if it is to occur at all, must take advantage of a flood tide, a millionth of a second's duration.

CONFIRMATORY EXPERIMENTS (6th March, 1888).

15. Two similar jars, each with dischargers, were connected as shown in fig. 6.

FIG. 6.



A spark at A now caused the distant jar to overflow easily, but had no effect on the near one. Similarly, a spark at C caused the jar distant from C to overflow easily, but had no effect on its own jar.

An A spark never caused a spark at C. Sparks occurred either at A or at C, according to which happened to be the narrowest gap, but not at both; and it was always the jar most distant from the spark that overflowed its lip.

16. The explanation probably depends upon the fact that when a spark discharges its near jar the charge from the distant one rushes forward, but, not being able to arrive in time, surges back violently and overflows. The effect can probably be imitated with a long water trough by momentarily opening and suddenly closing a trap-door at one end. It can certainly be observed in a lavatory where there is a constantly dribbling cistern for flushing purposes. By opening and suddenly closing one of the wash-basin taps a surging is set up in the connecting pipe, and the dribble becomes a periodic for a second or two, in synchronism with the period of longitudinal vibration of the water in the pipe.

Something apparently of the same sort has been quite recently observed with sinuously alternating currents by Mr. Ferranti in the Deptford mains. But whereas that case can be described as a long stretch of capacity with locally concentrated inductance, mine is a long stretch of inductance with locally concentrated capacity. Accordingly, while he observes an extra current-amplitude, I observe an extra potential.

The phenomenon in another form seems to have been first observed by Sir W. R. Grove, and fully explained by Clerk Maxwell (see 'Phil. Mag.,' for March and May, 1868). It was subsequently rediscovered by Dr. Muirhead, and explained by Dr. Hopkinson ('Journ. S.T.E.,' 1884). A note sent by me to the 'Electrician' for 24th April, 1891, contains a summary of the history and explanation.

Discussion of Overflow and Surging Experiments.

17. For the complete explanation of the overflow experiments, the static capacity of the long wire, and the momentum of the pulses rushing along it, must be taken into account, and a wire is more effective when insulated and charged than when lying on the ground.

It does act, however, even when lying on the ground, *i.e.*, when its magnetic momentum is all that can be supposed effective. But the ordinary theory of discharge oscillation will not account for the jar being thereby raised to a higher potential than it was at the beginning of the series; the amplitude of the vibration necessarily decreases. Hence it is probable that the fact of overflow does not prove that the entire potential of the jar is raised; only that the potential of the tinfoil edges is excessive. The charge is probably not uniformly distributed at the extremity of each swing. The fringe of sparklings above the edge of the tinfoil are well known whenever a jar is discharged; and overflow is merely an exaggeration of these sparklings, which usually leap up and subside. In fact they can be seen to jump higher and higher, as the spark is gradually increased, until the lip is leaped.

The idea of the pulses rushing along the connecting wires, and adding their momentum to the oscillation of the jar-discharge, suggests that there must be a best length for the connectors, viz., when the period of their pulses agrees with the period of oscillation of the discharge; and the fact that there is a best length is found experimentally.

The same length of connector is not equally effective with pint and gallon jars. A longer one is best for the larger jar; and if a connector be too long it does not promote overflow any more vigorously that if it were somewhat too short.

The damping effect of resistance no doubt partly comes in here as helping to account for the evil of unnecessarily long connecting wires; and no fine adjustment of length has been found necessary to bring out in a marked manner the surging effects.

If any experimenter should fail to obtain these conspicuously, he probably has his connectors too short or too long. It is advantageous, though not essential, to have the long wire insulated. It is essential to have it highly conducting. Iron is for these purposes by far the worst conducting metal, because it is magnetically throttled

Another small point is that good contacts aid in causing overflow; especially when the connecting wires are not long enough. Insignificant air spaces suffice to damp out some of the vigour of the subsidiary oscillation to which these effects seem due. With long massive leads, however, good joints are not of so much consequence.

(Parenthetically it may be remarked how well adapted the usual orthodox lightning conductor is to develop violent surging and splashing effects.)

Further Overflow and Surging Circuit Experiments.

18. Two jars standing side by side, and connected in parallel by long wires to the machine, sometimes both overflowed. Sparks taken at the jar knobs with ordinary discharging tongs had no such effect.

The tongs were sometimes arranged over the lip of a jar, so as to help its overflow if possible; but it was not easy to do this. Near the edge of each coating they had the best chance, but the splash usually preferred an immense jump through air over a glass surface to a much smaller jump through the discharging tongs. Overflow is evidently a very quick effect, and must occur in a hurry or not at all.

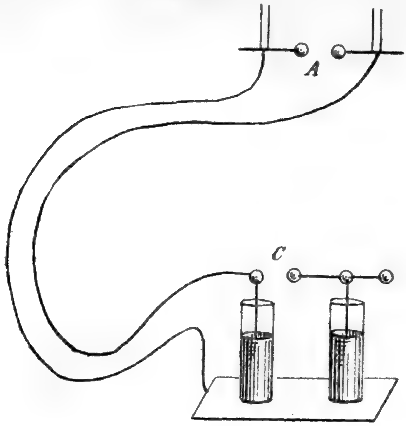
A couple of jars standing side by side on the same metal plate had a gap between their knobs as shown in fig. 7, and one of them was connected by long leads to the machine. It now often sparked across C into the second jar when an A spark occurred. But the second jar was not thereby charged. The charge just sprang into it and out again.

Connector without Self-induction.

19. Connected up a jar to the machine with a special anti-induction zigzag of tinfoil, folded to and fro in twenty long layers with several thicknesses of paraffin paper between. Could detect no effect on the jar overflow. It acted like a simple short circuit.

Tried, on the other hand, a high inductance coil, viz., the gutta-percha-covered bobbin of a Wiedemann galvanometer, with an iron-

FIG. 7.

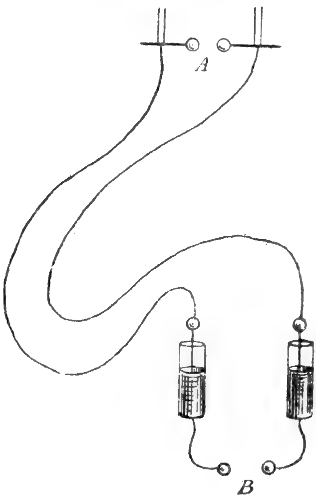


wire core inside: but its resistance was too high: it damped the oscillations.

Connector with Self-induction.

Interposed between machine and jars two thin wires round the room, and led the outer coats of the jars direct to a discharger, as in fig. 8.

FIG. 8.



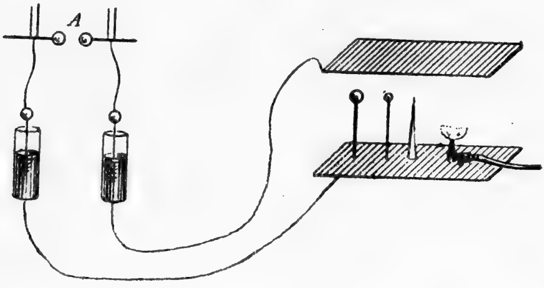
The jars being gallon jars, standing on wooden table. Compared A and B sparks; B was very long. Then substituted short wires for the long ones, and compared again. B was nearly as short as A. Readings follow :—

	Length of A spark.	Length of B spark.
Jars joined to machine by long wires ...	0·4 inch	2·2 inches
Short wires substituted	0·4 "	0·5 "

Overflow of Plate Condenser.

20. Connected a pair of tea-trays to the machine by long thick wires, and fixed them parallel to one another, keeping them asunder by glass or paraffin pillars; the jars standing on a wooden table, or being otherwise leakily connected so that they might charge. Every machine spark at A (fig. 9) caused long brushes, or sometimes remarkably long flashes between the plates.

FIG. 9.



A jar standing on bottom plate will receive a flash, but it will not necessarily be thereby charged; a slight residual charge may be found in it, but no more.

Points also get struck, just as noisily as knobs, and no more readily. Crowds of points, and knobs of all sizes, get struck equally well, if of the same height and all equally well connected to the bottom plate. The highest gets struck at the expense of the others. Often, however, several get struck at once. A gas-flame burning on the bottom plate gets struck at a much greater distance than does any metallic conductor. The weak hot-air column is precisely what this

overflow discharge prefers. It takes it in preference to a metal rod of twice the apparent elevation, and strikes down right through the flame.

But though it thus readily smashes a weak dielectric, it will not take a bad conductor. A wet string or water tube may, in fact, reach right up till it touches the top plate, and yet receive no flash, while the other things shall be getting struck all the time.

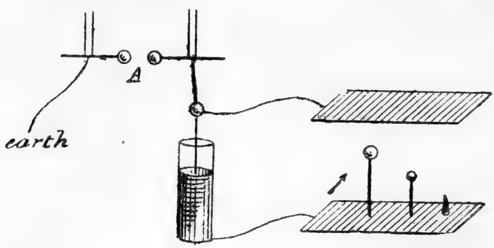
When the striking distance is too great for a noisy flash, a crowd of violet brushes spit between the top plate and protuberances on the lower plate: reminding one of some lightning photographs. The effect is still more marked if the top plate is a reservoir of water with a perforated bottom. The rain shower increases the length of these multiple gentle high-resistance purple discharges. Adding salt to the water tends to bring about the ordinary noisy white flash of great length.

Contrast between Path of Discharge under circumstances of Hurry and Leisure.

21. When the plates are arranged as in fig. 9, so that until an A spark occurs they are at the same potential and are then filled by a sudden and overflowing rush of electricity, all good-conducting things of the same height struck equally well, independently of their shape.

But when, on the other hand, the difference of potential between the plates was established gradually, as in fig. 10, so that the strain

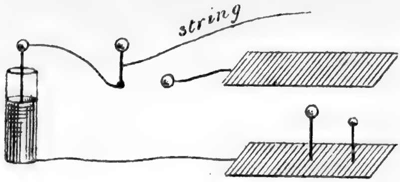
FIG. 10.



in the dielectric had time to pre-arrange a path of least resistance, then small knobs got struck in great preference to big ones, and points could not be struck at all, because they take the discharge quietly.

An intermediate case is when the charge and discharge of the top plate is brought about by pulling a lever over with string, so as to connect it with the jar, as in fig. 11.

FIG 11.



Sparking Distance between Plates in the Different Cases.

Terminal of rod standing on bottom plate.	Sudden rush caused by A spark, fig. 9.	Steady strain, fig. 10.	Intermediate case, fig. 11.
Brass knob 1·27 inch diameter.........	0·93 inch	0·90	0·67
Brass knob 0·56 inch diameter.........	0·93 ,,	2·95	1·4
Brass point.........	1·03 ,,	At 6 inches it prevented discharge until covered up with a thimble.	—

Unless the jars are large, compared with the capacity of the plates, even the conditions of fig. 9 will not make the rush quite sudden; and in that case points and small knobs do get struck more easily than large knobs and domes, especially when the top plate is negative.* But when the rush is really sudden, no difference as to sign manages to show itself; and even such insignificant advantage as the point happens to show in the first column of the above table disappears.

High resistance, interposed between knob and bottom plate in fig. 10, alters the character of the spark entirely, making it soft and velvety, but has no effect upon its length nor upon the ease with which its knob gets struck as compared with others connected direct. But the same resistance, interposed in fig. 9, prevents its being struck altogether.

In other words, sudden rushes strike good conductors, independent of terminal: steady strain selects sharp or small terminals, almost independent of conductivity; the violence of the flash being, however, by high resistance very much altered. The total energy is, doubtless, the same, or even greater with the quiet heating spark, because of concentration and no loss by radiation; but the duration

* This fact has been explained by Mr. Wimshurst, 'Journ. Inst. Elec. Engineers,' 1889, page 482.

of the discharge is what makes the difference. The spark through high resistance, instead of being alternating, can be seen to be intermittent (*i.e.*, multiple), when analysed in a revolving mirror.

There is no need in these sudden rush experiments for the long leads of fig. 9, though perhaps they add to the length of the sparks.

22. Sparks thus obtained from the outer coats of jars are convenient for taking under water, or to water; and the phenomena thus seen are singular, and sometimes violent.

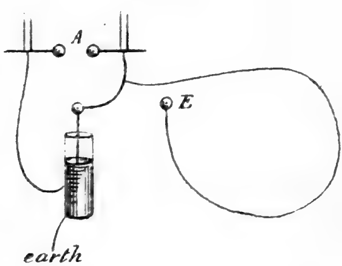
Water acts mainly as a dielectric under these circumstances, and, with small electrodes, such as the bared end of a gutta-percha wire, the water between gets burst with extraordinary violence: often breaking the containing glass vessel.

This arrangement of Leyden jars should be handy for blasting operations, because no specially good insulation of the leads is necessary.

Experiments on Surging Circuit Proper.

23. Although all the overflow experiments are controlled by electrical surgings, I have been accustomed specially to apply the name "surging circuit" to the case where sparks are obtained not between two distinct parts of a circuit, but between two points on one and the same good conductor, under circumstances when it does not form the alternative path to anywhere, and when it would ordinarily be supposed there was no possible reason for a spark at all. For instance, in fig. 12 the loop of wire round the room is a mere off-shoot or appendage of an otherwise complete and very ordinary arrangement, and yet a spark can occur at E whenever the ordinary discharge occurs at A; a spark, too, often quite as long, though not so strong, as the main spark at A.

Fig. 12.

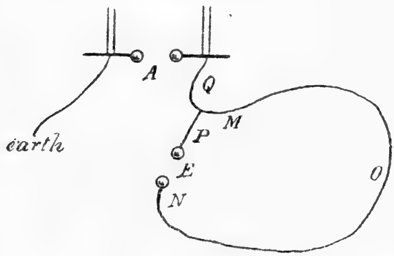


The jar is not essential to this experiment; and, in order to analyse it by inserting resistance at various places, it was modified to fig. 13,

and the following readings taken: first, with a thin copper wire, and then with a thick copper wire, round room. The $\frac{1}{4}$ megohm liquid resistance could be inserted at either M, N, O, P, or Q.

The A knobs used were the small ones of a universal discharger, 1·4 cm. diameter, and 2·4 cm. apart all the time (equivalent to 1·5 cm. spark-length between flat plates). The E knobs were those of a spark-micrometer, and were 1·96 cm. diameter.

FIG. 13.



		Length of E spark.	Character of E spark.
No. 19 copper round theatre.	Resistance inserted at P ..	0·819 cm.	Weak.
	No resistance inserted anywhere	0·597 ,,	Strong.
	Resistance at P again......	0·822 ,,	Weak.
	No resistance............	0·555 ,,	Strong.
	Resistance at M..........	0·571 ,,	Strong.
	No resistance	0·571 ,,	Strong.
	Resistance at M again.....	0·571 ,,	Strong.
	Resistance at N..........	0·423 ,,	Very weak.
	Resistance at O..........	0·621 ,,	Strong.
	No resistance............	0·536 ,,	Strong.
	Resistance at Q..........	No E spark at all, and A very weak.	
No. 1 copper round theatre.	No resistance............	0·524 cm.	Strong.
	Resistance at N..........	0·379 ,,	Very weak.
	Resistance at M..........	0·638 ,,	Strong.
	Resistance at Q..........	No spark at all, and A weak.	
	Resistance at P..........	0·793 cm.	E weak but A strong.

This table evidently shows that the main part of the E spark is the rushing of the charge in the N part of the wire back to the discharged A knob. It has two paths, through the wire *viâ* O, and direct across the spark-gap E. Most of it chooses E, except when there is high resistance at N or P. Resistance at O interferes but little, and in fact it may help more across E; and resistance at M must certainly

have this effect. Resistance at Q prevents any sudden effect of the A spark on the long circuit, and therefore never calls out a spark at E at all: the charged wire discharges leisurely through resistance at Q, and accordingly (there being no jar) the spark at A is quiet.

The fact in the table not immediately intelligible is the extra length of E spark caused by insertion of resistance at P; or, to a less extent, at O. It would appear to indicate the effect of surgings in the conductor, which accumulate a momentary opposite charge on one of the knobs before the one partitioned off by high resistance has had time appreciably to discharge.

Experiments on Recoil Kick.

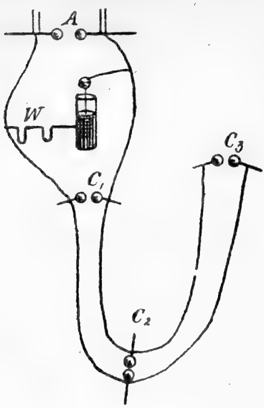
Early Observations (1 and 2 March, 1888).

24. Although the overflow experiments are evidence of the momentum of a reflected pulse, an idea which is intended to be conveyed by the term "recoil kick," yet I have been accustomed to apply the term specially to cases where reflexion takes place at the free end of a long wire, constituting an appendage or lateral extension, without forming any necessary part, of a discharging circuit. Usually a pair of similar wires were employed, and their ends were brought near enough for the momentum of the recoiling pulse, when spitting off from each wire, to bridge the interval and thereby cause a regular spark. If the wires were too far separate, a momentary brush leaped from each end and subsided again. Occasionally these brushes extended over a considerable length of wire, giving them a peculiar luminous appearance at each discharge. The fact that the brush was an up-rush and subsidence was shown by the similar appearance of both the wires, and by the fact that if a spark from either wire was taken into a jar the jar was not found to be charged by it.

25. The brush or sparking out from the wires, which I call the recoil kick, is most marked at certain places on those wires; and usually at the distant ends. This was what called attention to the effect (see above § 3). I find that Mr. A. P. Chattock obtained the first direct evidence of it, in some experiments with my apparatus which he made in my absence on March 1, 1888. The plan of the particular connexions used by him (fig. 14) has no importance, but it sufficed to show how much more readily a long spark could be obtained at the far end of long wires than at the near end; and Mr. Chattock was quite clear about the effect being due to reflected electric pulses or stationary waves in the wires, and was prepared to look for evidence of nodes and loops if the wires had been long enough.

In fig. 14, W is a high liquid resistance, the two long wires are the

FIG. 14.



thin copper and thin iron round room, and 1, 2, 3 are three alternative positions of a universal discharger, while A are the knobs of a Voss machine. Spark lengths are given in inches, but there is no importance in their absolute values.

Length of A spark needed to precipitate a spark between knobs of discharger *c* in its several positions.			Length of spark between knobs of discharger thus obtained. Called C.
Position 1.	Position 2.	Position 3.	
0·32	0·19	0·16	0·32
0·51	0·27	0·25	0·62
0·42	0·24	0·22	0·48
0·17	0·14	0·14	0·17

26. Next day I went on with these observations, replacing the liquid resistance W (which was useless) by a wire, and ordinarily using two jars in series instead of one, connecting their knobs one to each wire (more nearly as shown in fig. 15), and connecting their outer coats together and roughly to the earth by standing them on the same sheet of tinfoil on a wooden table. And because the knobs of machine and of discharger were not the same size, they were first compared by letting an ordinary discharge choose between them. They offered equally good paths when A = 0·45, C = 0·54 inch. The discharger was put in one or other of two positions: bridging

the long wires close to the jars (C_1), and bridging them at their far ends (C_3), fig. 14. The position C_1 manifestly does not essentially differ from A; the position C_3 is the interesting one. Of course if C were too short, the main spark occurred there instead of at A, but if the main spark occurred at A, a much longer supplementary or recoil kick spark often occurred at C_3, especially when the capacity of the jars and the length of the wires were suited to each other. As the following table shows:—

	Length of sparks.		
	A.	C_3.	C_1.
Without any jars	0·3	0·3	—
With small Voss jars	0·3	0·35	—
With pint jars (two in series)	0·24	0·42	—
Lengthen A till sparks just choose C instead	0·39	0·42	—
Shorten A till recoil sparks just fail at C	0·22	0·42	—
Get maximum C spark	0·42	0·63	—
" " "	0·45	0·75	—
" " "	0·45	—	0·47
Without any jars again (size of knobs accounts for this slight difference)	0·44	0·52	0·49
With the two pint jars, in parallel, shifted to the far end of wire near position 3, with overflow knobs to represent C_3	0·44	0·45	0·49
Same arrangement of jars shifted back to near position 1	0·44	0·78	—
Get the sparks at C_3 instead of as A	0·44	0·49	—
Arrangement as at first	—	0·49	—
Pair of gallon jars in series	0·45	1·09	—
Large condenser (0·02 mfd.)	{0·45	0·59	—
	—	0·49	—

Thus the large condenser is as much too big as the Voss jars were too small. The gallon jars seem to show the effect best. They were therefore replaced, but this time insulated from the earth by standing them both on the same insulating stool with tinfoil top. Very long recoil sparks could now be got.

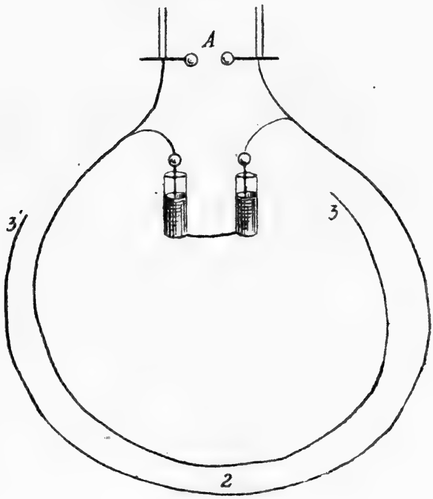
	A.	C_3.
Gallon jars in series on insulating stool	0·44	1·5
Another experiment	0·44	1·35
Big knobs put on the discharger	0·44	1·38

And at greater distances, when no regular spark occurred at C, there was still a brush discharge there at every A spark.

Joining up a small jar to the C_3 terminals, the long sparking and brushing there ceased.

Without this shunt jar, however, and with the terminals well separated, the wires glowed at every A spark along a considerable portion of their length, looking thick and fuzzy with momentary luminosity. (The wires were the long thin (No. 18 B.W.G.) copper and iron round the room: those two being up and handy for the experiment. They show the luminous appearance better than either thicker or much thinner ones.) With a small condenser the effect was not great, and with a very big one it was also not great; but with a single gallon jar the glow on the wires extended more than half way round the theatre, and a pair in series (*i.e.*, half the capacity) seemed to do even a trifle better.

Fig. 15



27. To see if the proximity of the opposite wires assisted the effect one of them was reversed, so that the plan was as in fig. 15; but the far ends of both wires got luminous as before (although the luminous portions were now on opposite sides of the room), and the luminosity extended from 3 to 2 on the one wire, and from 3′ to 2 on the other.

28. A jar was held in the hand near either glowing terminal, for the wires to spark into. They kept on doing so, but the jar was not charged, showing that they sparked in and out again.

This is characteristic of what I have elsewhere examined and called "side flash."

Estimation of Wave-length.

29. Although there is nothing precisely metrical about these experiments, as so far conducted, it is well to notice that an approximation to the self-induction of the main discharge circuit can be obtained from them. For the capacity of the two gallon jars in cascade is about 28 K metres; and if the length of the wires which give the best recoil be taken as half a wave-length, the waves emitted are 60 metres long. So the inductance of the discharge circuit can be got from $2\pi\sqrt{(28\mathrm{L}/\mu)} = 60$; whence $\mathrm{L} = 3{\cdot}2\mu$ metres.

This is too small, showing that the waves are longer than 60 metres, and that the most appropriate capacity for these particular wires is something less than that of the two gallon jars in cascade.*

Historical Observations.

30. This evidence of the existence of electro-magnetic waves seemed to me of considerable interest, because I had been for some years contemplating the production of radiation by direct electro-magnetic experiments, the difficulty being their detection, *i.e.*, the proof of their existence.

My early notions, described to Section A of the British Association at York (1881), were directed towards the ambitious attempt of trying to make the waves short enough to be visible, at least to a thermopile or to some chemical detector. But two years later, at Southport, Fitzgerald pointed out that a discharging Leyden jar must emit radiation, and that though its waves would be yards or miles long, yet it might not be hopeless to prove that they were waves by obtaining interference phenomena.

Some of Lord Rayleigh's large-scale interference experiments with sound waves, exhibited to the Royal Institution on January 20, 1888 ('Nature,' vol. 38, p. 208), re-awakened in me the hope that such experiments were possible, and the desire to try them. And now, simply by attaching long wires to a discharging Leyden jar circuit, the waves had become without trouble conspicuous. One had only to lengthen the wires enough, and to look at them in the dark, to see by the brushes the nodes caused by the interference of the direct and reflected pulses surging to and fro in the wires; to see in fact the waves themselves, and to measure their length in a manner precisely analogous to the well-known experiment of Melde.

* [Or else that each wire behaves like an organ-pipe open at one end only, and so is a quarter of a wave long.—June, 1891.]

True that the Melde experiment does not measure the wave-length in air, and so also the observation of Mr. Chattock and myself would only measure the wave-length on wire; but it had already been shown by Mr. Heaviside among others, by Kirchoff also (though I did not know of Kirchoff's work), that pulses travelled along insulated non-magnetic wires at the same speed as waves through air, or at a speed only insignificantly less. In fact Mr. Poynting has taught us to regard all these effects as conveyed through the air, *i.e.*, by the ethereal medium, in a manner only very subordinately affected by the material of the conductor.

Hence the waves guided by long isolated wires and measured in recoil kick experiments ought to be the same length as, or only slightly shorter than, the true ether-waves spreading out from the oscillating circuit into space.

The fact that electric waves could be thus detected and measured, I stated at the Society of Arts, on March 17, 1888, and published more precisely in the 'Phil. Mag.' for August, 1888; but to this latter I appended a footnote to say that in the current number of Wiedemann's 'Annalen,' viz., that for July, the same year, there was a paper by Dr. Hertz, describing some experiments he had made at Karlsruhe, whereby he had detected the waves in free space: a research which in the following September was enthusiastically proclaimed to the world by Fitzgerald, at Bath. At the same meeting I described in general terms my detection of the waves on the surface of conducting wires. It appears that Hertz began, much as I had done, by the observation of surging circuits; for, using a coil instead of an inductive machine, and attaching to one terminal a nearly closed rectangle, he observed it spark across the gap. In this observation also he had the start of me, for his first paper appeared in 1887; and in his rapid development of it, in the comparative freedom from students of Karlsruhe, he struck on the influence between one circuit and another across space, and so made the astonishing discovery that the radiation in air was intense enough to cause sparks in conductors upon which it fell.

This same discovery would have been made by the audience at the Royal Institution on the evening of March 8, 1889, if it had not been made before; for, during a lecture on Leyden jars, every time one was discharged through a considerable length of wire, the heavily gilt wall paper sparkled brightly, by reason of the incident radiation.

The achievement of Hertz is well known, and it is only the customary interest attaching to circumstances connected with what will probably be regarded as an epoch in electrical science that constitutes my excuse for making the above statement.

One point, about which there has been some controversy, my expe-

riments do make clear, viz., that the velocity of a pulse along an isolated thin copper wire is practically identical with the speed of light; in accordance with the theory based on Maxwell, and previously mentioned. Hertz at one time stated, as the result of some of his experiments, that there was considerable discrepancy between the speed of waves along wires and of waves in free space; and, though my own experiments were (to me at least) conclusive in the opposite direction, yet as they had not been published in detail, they could not be properly taken into account. The supposed discrepancy, however, had the good effect of leading Professor J. J. Thomson to make several interesting experiments.

Quantitative Recoil Kick Experiments (May, 1888).

Description of Wires used.

31. In order to make real measurements of wave-length, a circuit was carefully prepared, consisting of two copper wires (about No. 17 B.W.G.), 15 cm. diameter, stretched parallel to one another, half a metre apart, by silk suspenders.

They lay parallel to the theatre table, *i.e.*, north and south; but the room was not big enough for them to be wholly straight, so after travelling the length of the table horizontally they were taken a few feet vertically up, then back over head, and down again to the spark micrometer, according to the plan of fig. 16, nowhere being taken near any wall or other surface. Their total lengths were

$$1526+28{\cdot}5+16 = 1570{\cdot}5 \text{ cm.}$$
$$1529+36{\cdot}5+16 = 1581{\cdot}5 \;\;,,$$

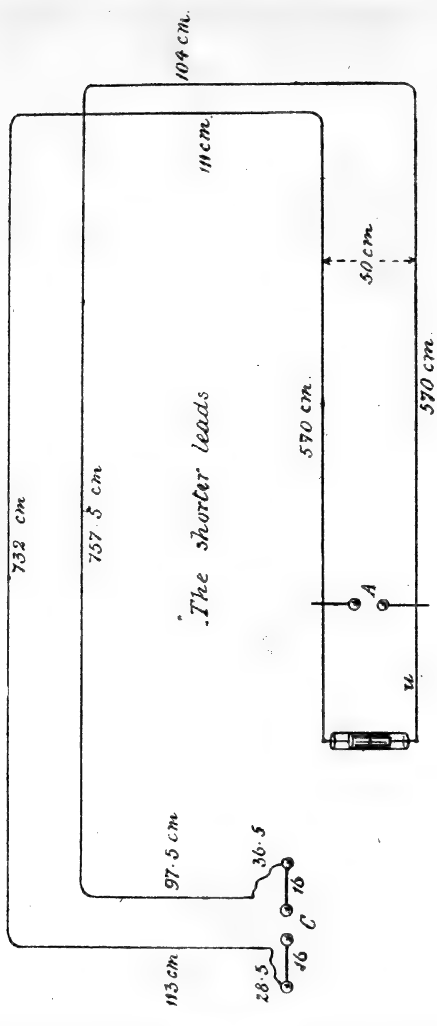
so each may be taken as $15\frac{3}{4}$ metres long, and the wave length corresponding to their fundamental oscillation period as $31\frac{1}{2}$ metres, corresponding to about 10,000,000 vibrations per second.

When used alone, these are spoken of as "the shorter leads," because it was very soon necessary to supplement them by a similar pair of No. 17 copper wires half a metre apart, suspended similarly, but in an east and west direction, and used as extensions. When joined up in series with the former pair of wires the whole is spoken of as "the longer leads." The additional portions measured 2263 and 2247 cm. respectively. Hence the entire length of each of the longer leads may be taken as 38·2 metres, and the wave-length corresponding to their recoil as $76\frac{1}{2}$ metres, or say, 4,000,000 vibrations per second.

The total resistance of the "shorter leads" was 0·78 ohm, and of the "longer leads," 1·95 ohms.

The static capacity of the longer leads I estimate, by the formula

Fig. 16.



Kl÷4 log b/a, as 147 K cm. altogether, or K/26 per unit length; and their self-induction similarly, for currents which keep wholly to the outer skin, as 100,000 μ centimetres, or 26 μ per unit length. The electrostatic capacity of the "shorter leads" is 61 cm.

Plan of Experiment.

32. At the far end of these leads (either "the longer" or "the shorter") was arranged the spark micrometer, a micrometer screw

arrangement with millimetre thread, and head divided into 400 parts, made for reading Newton's rings, but supplied for present purposes with a pair of knobs 1·940 and 1·965 cm. diameter, on insulating glass pillars. These constitute the C spark gap at which the effect of the recoil kick is to be observed.

At the other end of the leads is arranged the condenser, usually a pair of jars back to back in one line, so as to close the circuit in a simple geometrical manner, and the Voss jars being at first used, *i.e.*, the small jars forming part of the Voss machine.

The A knobs at which the exciting spark is taken are constituted by the universal discharger, which, standing on a block under the two parallel wires above it, and connecting them together through an air gap, can easily be moved along the table to and fro, always in contact with the wires above it; and its distance from the jars at one end of the wires can be readily measured. This distance is called u, and is indicated in fig. 16.

The distance between the A knobs was supposed to remain constant, but to avoid any uncertainty, and to eliminate the effect of the different size of its knobs, the virtual length of the A spark was measured with the spark-micrometer by bringing its knobs near enough to just shunt out the discharger, so that half the sparks chose one and half the other. The distance between the C knobs under these circumstances is entered in the table as the virtual length of the A spark. They are then separated further, so that the spark occurs every time at A, but it does not cease at C until they have been widened distinctly. The maximum distance to which they can be separated without causing the C spark to altogether cease is then read, and the excess of the one reading over the other constitutes the "recoil kick." This procedure was then repeated for another position of the discharger A.

The plan of operation usually was to begin with the discharger close to the jars, and to move it away along the leads 5 cm. at a time, reading in each position the minimum and maximum C sparks, as just explained, the maximum reading being that at which C began to fail. Tuning is more easily done by thus varying the inductance of the discharge circuit than in any other way.

A short range would have been sufficient to give the position at which the maximum recoil kick occurred, but an extensive range was often used with the idea of getting indications of harmonics.

Measurement of Capacity of Jars used.

33. Rough estimates of the jars employed in this and similar experiments could be made in various ways, and no great accuracy is worth aiming at with ordinary shapes of jar, because of the difference between the circumstance under which they are used from those

under which they are measured. Nevertheless, in order to get a fairly good measure of their capacity, a standard condenser was made, with which they could be compared.

Standard Air-condenser.

A couple of plates of carefully selected plate glass, as used for mirrors, about 2 feet square, were silvered chemically on both faces, secure connexion between the two faces round the edge of the top plate being made. A circular cut or clean scratch, 1 mm. broad and 53·04 cm. inside diameter, was then made on the under surface of the top plate so as to isolate a trap-door portion. A hole previously drilled through the centre of the top plate, and silvered inside, permitted conductive access to the central area; and the borders of the plate acted as guard ring. The silver was cleared away from a small patch near the centre of the upper surface of the top plate, and a glass tube cemented on permitted the trap-door terminal to emerge in a well-insulated manner. Two other terminals, one attached to the bottom plate, and the other to the general surface of the top plate (the guard-ring terminal), were provided.

Four glass distance-pieces with wide bevelled edges (to improve insulation) were carefully cut out of one piece of glass, and their thickness was measured with a spherometer as 0·12083 inch.

The top plate was supported on these, near its four corners, and the whole placed in a suitable box with artificially dried atmosphere, the bottom plate being similarly supported near its corners, so that whatever bending there was might result in concentric surfaces. The electrostatic capacity of the trap-door was thus 572·9 cm.

There was some difficulty with the insulation of the trap-door portion, and the main leak was traced to dust, viz., fine fibres of some length, which settled on the bottom plate and bridged the interval between it and the top one. The narrow gap between trap-door and guard-ring, being of course carefully cleaned, was not found to leak anything like so much as one of these fibres. By care, the causes of leak could be minimised, and a special key was constructed whereby the trap-door could be discharged through a ballistic galvanometer the merest instant before the guard-ring was discharged to earth. In fact, by a screw adjustment the two events could be made simultaneous.

34. A set of 144 secondary Planté cells, made by bending strips of lead over small glass vessels standing in a sort of test-tube rack, having been charged in twelve sets of twelve each, were connected in series and used to fill the condensers with.

Sometimes different kicks were obtained with the whole series of cells; sometimes the same kick was imitated by tapping off a certain

number of them, the same number being taken in different parts of the battery to secure fair uniformity.

The following are the estimates of capacity made from these observations, the standard air-condenser being taken as 573 cm. :—

Capacity Measurements.

	Electro-static capacity.
Two gallon jars in series	2800 cm.
One of them	5640 „
Two pint jars in series	660 „
One of them	1280 „
The other	1360 „
One Voss jar	214 „
The other Voss jar	357 „
No. 1. Sliding tube-condenser; length in use 3 cm.	80 „
„ „ „ 6 „	120 „
„ „ „ 9 „	177 „
„ „ „ 12 „	226 „
„ „ „ 15 „	258 „
„ „ „ 18 „	290 „
No. 2. „ „ „ 15 „	272 „
„ „ „ 20 „	338 „
„ „ „ 28 „	450 „

The two jars supplied with the Voss machine happen to be, unfortunately, unequal. Their capacity in series was rather small to actually measure satisfactorily: so I take it as 134 cm.

First Approximation to Inductance of Circuit.

35. In order to estimate the inductance of the discharge circuit, it is necessary to know the following data:—

Thickness of the No. 17 wires	0·15 cm.
Thickness of A discharging rods	0·60 „
Thickness of rods inside Voss jars	0·98 „
Thickness inside other jars	0·6 „

Now, the discharge circuit consisted of a rectangle with one pair of opposite sides made by certain length, u, of the No. 17 wires 50 cm. apart, and with the other pair of opposite sides made by discharge and jar rods, 50 cm. in length, and separated by a distance, u.

I am not prepared to calculate the inductance of this rectangle precisely, but when approximately square it makes but little difference whether I reckon it as a pair of parallel wires of length u, distance 50, and diameter 0·15, plus a pair of parallel rods of length 50,

distance u, and diameter 0·8; or whether I reckon it as a circle of perimeter $2\ (50+u)$ and average thickness 0·3. For cases where the rectangle is elongated, the former approximation is best, so I use it in preference to the other always; reckoning the self-induction, therefore, as

$$L/\mu = 26u + 200 \log \frac{u}{0{\cdot}4} \text{ cm.} \quad \ldots\ldots\ldots\ldots \quad (24),$$

Its real value will be somewhat greater than this.

36. In the following tables the experimental data were obtained and recorded carefully. The notes appended to each, concerning the amount of agreement between calculation and experiment, are capable of further refinement: but they suffice to show that the discrepancies between calculation and observation are as small as could be expected, and that, to a first approximation, experiment and theory agree; in other words, that if the velocity of a pulse, along thin isolated copper wires, differs from the velocity of light, it does not differ to any considerable extent.

Results (12th May, 1888).

37. Two Voss Jars end to end, as shown, with "Shorter Leads."

Length u.	Min. C spark, *i.e.*, virtual length of A spark (in mm.).	Max. length of C spark (in mm.).	Excess of max. over min. (C—A) or recoil-kick (in mm.).
cm.			
5	9·15	—	—
10	9·10	—	—
15	9·05	9·39	0·34
20	9·10	11·25	2·15
25	9·00	11·27	2·27
30	9·05	12·00	2·95
35	9·26	12·90	3·64
40	9·26	11·09	1·83
45	9·22	10·20	1·98
50	9·29	9·38	0·09
60	9·15	9·24	0·09
90	9·25	9·60	0·35

Here the maximum occurs somewhere about the 35 cm. distance, probably on the hither side of it; so we may judge that the value $u = 34$ is about the place where the waves emitted agree with twice the length of the "shorter leads," *i.e.*, are $31\frac{1}{2}$ metres long.

To see how this agrees with calculation, we must decide how much of the capacity of those wires ought to be added to the capacity of the jars in calculating the period of the discharge oscillation. If we

include none of the leads in the discharged capacity, S/K = 134 cm.; if we include the whole, S/K = 195 cm.

As for the self-induction, we get that approximately by putting $u = 34$ in the expression (24), which gives it as 1768 cm.

So the calculated wave-length is, with the whole of the leads capacity, $2\pi\sqrt{(195 \times 1768)} = 3690$ cm. or 37 metres; and the agreement is not very good. If the leads capacity is not supposed effective in forcing the vibrations, its charge being merely forced to vibrate by the jar discharge, then the calculated wave-length is 31 metres, which is suspiciously accordant with observation.

38. Single Voss Jar,* still with "Shorter Leads."

Length u.	Virtual A spark.	Max. C spark.	Recoil kick.
cm.	mm.	mm.	mm.
5	9·00	13·70	4·70
10	9·11	12·68	3·57
15	9·07	12·01	2·94
20	9·03	11·66	2·63
25	9·10	11·50	2·40
30	9·10	11·20	2·10
35	9·12	11·32	2·20
45	9·11	11·45	2·34

Here the maximum is evidently off the scale; so that, to show it, the discharging circuit requires to be still further diminished in size: which is impracticable. As to calculated wave-length, the lowest estimate makes it 30 metres, when $u = 5$: the highest estimate makes it $32\frac{1}{2}$ metres, either of which is so far accordant with observation.

Two Pint Jars in Series, with "Shorter Leads."

Although the back kick was now fairly vigorous, no particular evidence of its being greater at one place than in another was observable. Evidently the leads are too short. Hence added the extensions already described (§ 31).

* Probably the one with the larger capacity of the two, but unfortunately not quite sure of this.

39. Two Pint Jars end to end, "Longer Leads" (14th May, 1888).

Length u.	Virtual A spark.	Max. C spark.	Recoil kick.
cm.	mm.	mm.	mm.
8	9·52	15·14	5·62
10	9·45	16·24	6·79
12	9·63	15·88	6·25
15	9·45	15·85	6·40
20	9·73	19·20	9·47
30	9·90	27·73	17·83
40	9·85	18·25	8·40
50	9·91	13·75	3·84
70	9·96	12·73	2·77
90	9·85	12·99	3·14
125	—	13·28	—

Here the sharpness of the maximum is so great as to make it probable that $u = 30$ happens to hit the right place pretty exactly.

The calculated wave-length for this value of u (remembering to substitute 0·3 for 0·4 in equation (24) because of the less thickness of these jar-rods) is $2\pi\sqrt{(1700 \times 721)} = 7000$ cm., or 70 metres; a trifle less if the leads capacity is omitted; whereas the double length of the recoiling wires indicates $76\frac{1}{2}$ metres.

This is not very good, and I hope that better calculation applied to the data will give a better result. The self-induction as estimated is almost certainly less than the true: but how much less I am unable to say. Another uncertainty is the appropriate capacity of the jars for these high frequencies. It is not likely that the K for the glass will be just the same for 4,000,000 vibrations a second as it is under steady strain.

40. Single Voss Jar, with "Longer Leads."

(Knobs of discharger set exactly 1 cm. apart.)

Length u.	Equivalent of A Spark.	C spark.	Recoil.
cm.	mm.	mm.	mm.
5	9·27	13·38	3·41
10	9·26	15·20	5·94
15	9·30	16·57	7·27
20	9·50	14·42	4·92
25	9·50	13·88	4·38
30	9·43	13·41	3·99
35	9·56	13·40	3·86
40	9·52	13·60	4·08
50	9·50	13·63	4·13

Here the maximum is not quite so well marked, and it is a little uncertain on which side of the 15-cm. position it lies.

Take $u = 15$ however, and add the capacity of the leads, 147, to that of the jar, 357, and the calculated wave-length comes out 47 metres; or, without the capacity of the leads, $39\frac{1}{2}$ metres.

This is hopelessly different from $76\frac{1}{2}$ metres, but it is probable that it is the upper octave. To make the fundamental wave-length 79 metres would require u to be about 130 cm.

It is evidently desirable in these experiments that the jar capacity shall be much greater than the leads capacity; for when the two capacities are comparable in size, the recoil kick, although it still exists, has no very well defined maximum.

It would also be desirable in future experiments on the same plan to use, as dielectric in the condenser, a substance, like paraffin, whose specific inductive capacity may be depended on as fairly constant.

Experiments with Sliding Condenser.

41. The following experiments, made with an adjustable condenser, were intended to examine the effect of varying the capacity as well as the self-induction; also to see what evidence of harmonics could be detected.

The condensers used were each of them a pair of silvered tubes sliding into one another; their capacity, as the inner tubes were pushed into six different graduations, being measured above (§ 34).

The discharge circuit was a pair of No. 17 B.W.G. copper wires each 2 metres long, stretched horizontally on glass pillars, parallel to one another and 10 cm. apart. One of the tube condensers was arranged as a bridge at one end of these wires; and the discharger, with its knobs set 1 cm. apart, was used as a movable bridge and set at different measured distances from the condenser.

Charging from the machine was done through wooden sticks, so as not to introduce unknown capacities. The "longer leads" lead from the coats of the tube-condenser to the spark-micrometer, where the equivalent A spark and the maximum C or recoil spark were observed.

The numbers entered in the following table are the excess of the max. C spark over the virtual A spark, *i.e.*, C spark-length minus A spark-length, in millimetres. The maximum occurs in the different columns at about $u = 50$, say 35, 30, 25, 20, and 15, respectively.

42. Summary of Experiments with Tube-condenser No. 1.

("Longer Leads.")

Length of A spark mm...............	9·57	9·32	9·32	9·5	9·5	9·57
Distance of A knobs from condenser (the length u).	Recoil kick, or C − A (in mm.).					
	3 cm. in use.	6 cm. in use.	9 cm. in use.	12 cm. in use.	15 cm. in use.	18 cm. in use.
cm.						
6	—	—	—	6·10	7·05	8·60
10	3·20	5·28	7·10	8·07	10·00	10·93
15	—	—	—	10·07	11·60	12·33
20	3·80	6·63	9·58	10·86	12·10	12·18
25	—	8·37	—	11·15	11·50	—
30	5·30	9·03	11·00	11·00	11·15	12·18
35	—	8·85	—	—	—	—
40	5·73	8·22	10·15	10·45	10·45	—
50	6·40	7·88	9·68	10·17	9·87	10·80
60	5·20	7·16	9·10	9·82	9·90	—
70	6·26	6·78	9·00	—	10·05	10·38
80	6·40	6·40	—	9·00	—	9·31
90	4·45	—	8·58	—	9·00	—
100	4·03	6·27	—	8·05	7·97	7·84
150	—	—	7·10	7·00	7·82	7·18

The readings were not all taken on the same day; and the length of the equivalent A spark, as read on different days, is recorded at the top of each column. The wave-length is the same in each case, and rough calculation makes it probably about 27 metres when the kick is at a maximum. This may be supposed to be the second harmonic of the vibration period of the long leads.

43. Summary of Experiments with Tube-condenser No. 2.

("Longer Leads.")

Equivalent length of A spark, 9·22 mm. (June, 1888).

	Recoil kick, or C − A (in mm.).			
Distance of A knobs from condenser (u).	(1) 15 cm. in use.	(2) 15 cm. in use.	20 cm. in use.	28 cm. in use.
cm.				
5	6·04	6·68	8·18	9·65
10	9·00	9·18	11·04	12·28
15	10·35	10·78	11·90	12·48
20	12·05	11·52	12·18	12·28
25	10·90	11·15	11·25	11·90
30	11·43	11·19	11·23	12·08
40	10·20	10·93	11·04	12·78
50	10·15	10·35	—	11·89
60	—	—	10·43	10·70
70	9·68	10·35	—	10·03
80	—	—	9·73	9·22
100	8·28	9·03	8·18	9·48
120	—	—	8·98	10·03
130	7·15	8·58	—	—
150	6·34	8·28	7·35	10·33

The two sets with 15 cm. of the condenser in use are on different days, the only difference being that in (2) the discharge circuit was turned round 180° so as no longer to be beneath the longer leads at a possibly interfering distance. The absolute reading differs, but the maximum occurs in the same place.

The maxima in all this set are by no means well-marked, but there is an indication of a second maximum in the last column, at about $u = 40$; and of still another near the bottom. Taking the self-induction of the discharge circuit as $19\frac{1}{2} \times 45$, and the capacity of the jar as 450, the calculated wave-length for $u = 40$ comes out $38\frac{1}{2}$ metres, or just the first harmonic of the leads-vibration.

The second harmonic occurs at $u = 15$; with wave length $26\frac{1}{2}$ metres. There is a sign of another maximum near 150; and calculation places a fundamental vibration, agreeing in period with the leads, at about $u = 167$.

II. "On a Determination of the Mean Density of the Earth and the Gravitation Constant by means of the Common Balance." By J. H. POYNTING, D.Sc., F.R.S., Professor of Physics, Mason College, Birmingham. Received May 13, 1891.

(Abstract.)

In a paper printed in the 'Proceedings of the Royal Society,' No. 190, 1878, an account was given of some experiments undertaken in order to test the possibility of using the common balance in place of the torsion balance in the Cavendish experiment. The success obtained seemed to justify the continuation of the work, and this paper contains an account of an experiment carried out with a large bullion balance, in place of the chemical balance used in the preliminary trials. The work has been carried out at the Mason College, Birmingham.

The Principle of the Experiment.—The immediate object of the experiment may be regarded as the determination of the attraction of one known mass on another. If two spheres, of masses M and M′, have their centres a distance d apart, the attraction is, according to the law of gravitation, GMM'/d^2, where G is the gravitation constant. Astronomy justifies the law in certain cases as regards M'/d^2, but does not give the value of G or M, except in the product GM. To find G we must measure GMM'/d^2 in some case in which both M and M′ are known. Having found G, we may determine the mean density of the earth, for, assuming that it is a sphere of radius R, the weight of any mass M′ at its surface is

$$G \times \tfrac{4}{3}\pi R^3 \Delta M'/R^2$$
$$= \tfrac{4}{3} G\pi R\Delta M'.$$

But if g is the acceleration of gravity the weight of M′ may be expressed as $M'g$. Equating these values, we get

$$\Delta = \tfrac{3}{4}\frac{g}{G\pi R}.$$

Method of Using the Common Balance.—With the length of beam used (about 123 cm.) a differential method was applicable, in which the attraction on the beam was eliminated. Two spherical masses of lead and antimony, about 21 kilos. each, were hung from the two arms of the balance, so that their centres in the first position were about 30 cm. above the centre of a large attracting mass, a sphere of lead and antimony about 153 kilos., placed on a turntable,

so that it could be brought in turn immediately under either of the suspended attracted masses. A balancing mass of half the weight, and at double the distance from the centre of the turntable on the other side, was found necessary, so that the centre of gravity should be in the axis of rotation. Before this was used, the ground level was seriously altered by the rotation of the turntable. The attraction of the balancing mass was calculated and allowed for.

The alteration in the weights of the attracted masses, due to the motion of the attracting masses from one side to the other, was the quantity to be measured. When this was determined in the lower position of the attracted masses they were raised to about double the distance, and the attraction again determined. The difference eliminated the pull on the beam, suspending wires, &c. To lessen the effect of want of homogeneity or sphericity in the masses, or of want of symmetry in the turntable, the masses were all inverted and changed over each to the other side, and the weighings repeated.

The position of the beam was determined by the reflection of a scale in a mirror used with "double suspension." The mirror was suspended by two silk threads, one attached to the end of the ordinary pointer about 60 cm. below the central knife edge, the other parallel to it, being attached to a fixed support. The mirror turned through an angle about 150 times as great as that through which the beam turned, and one scale division corresponded to an angle of tilt in the beam of about 2/15ths of a second.

The value of a scale division was determined by the use of two equal centigram riders which could be placed on or taken off wire frames representing the scale pans of a small subsidiary beam, 2·5 cm. long, fixed parallel to and at the centre of the large beam. When one rider was placed on one supporting frame the other was at the same instant lifted off the other frame.

The balance was left free throughout a series of weighings, and no moving parts of the apparatus were connected with the case.

The values obtained are as follows:—

$$\text{The gravitation constant G} = \frac{6{\cdot}6984}{10^8}.$$

$$\text{Mean density of the earth } \Delta = 5{\cdot}4934.$$

In the paper a description is given of a new form of cathetometer used to measure the diameters of the masses.

III. "On the Pressure of Wind on Curved Vanes." By W. H. Dines, B.A. Communicated by the Meteorological Council. Received May 14, 1891.

In June, 1890, a paper* was presented to the Royal Society showing the results of some experiments upon wind pressure upon an inclined surface, and I now give an account of some supplementary work upon the same subject which has been done during the past winter.

The apparatus was the same as that previously described, with the exception of the actual pressure plate, and precisely the same method of observation has been adopted.

Instead of a flat wooden plate, a piece of sheet metal 1 foot square has been used, the metal being bent so as to form a portion of a cylinder, the curvature of which was easily varied by drawing the opposite edges more or less together by means of two fine wires. The plate was attached to the lever of the apparatus by about 13 in. of 1-in. brass tube, the tube passing a little more than half way across the back of the plate. It is evident that the tube must interfere with the free passage of the air over the back of the plate, but some kind of support behind cannot be avoided.

In certain positions, experiments could not be made on account of the unsteadiness of the motion, and the consequent fluttering of the sheet metal. There was no trouble in getting the value of the pressure in these positions, but the vibratory motion was often so violent that it tore the metal, almost as though it were paper, and soon rendered the plate useless. These positions are all marked * in the tables; and the corresponding values are more or less uncertain, because, as soon as the vibrations were apparent, the engine was stopped as quickly as possible to avoid the trouble of having to obtain a new plate.

As in the preceding paper, 100 has been taken to represent the moment of the pressure upon one sq. ft. exposed normally at 1 ft. from the axis, and all other moments are expressed relatively to this.

The results obtained are given in the following tables, the diagram at the head of each table showing the form of surface to which it applies. No attempt has been made to eliminate the effect of the eddy from the frame of the apparatus, a full discussion of which will be found in the paper referred to. The values given also include the pressure upon the supporting arm. This is counterbalanced in the normal position, but must have an increasing effect as the angle of incidence increases, and for this reason it has been considered useless to carry the experiments much beyond an angle of 60° or 70°.

* 'Roy. Soc. Proc.,' vol. 48, p. 233.

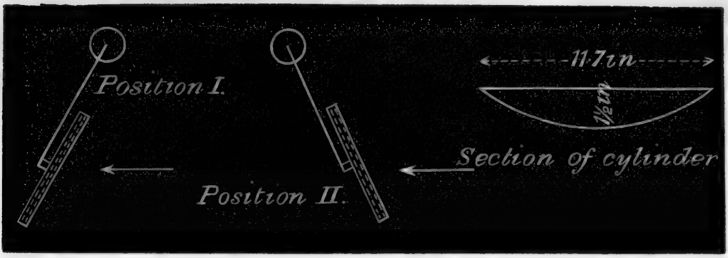
The diagrams showing the way in which the normal component of the pressure varies with the angle of incidence have been obtained by taking the mean values from positions I and II. In drawing the curves, the want of observations at the intermediate angles was felt, but I do not think that further experiments would greatly modify the forms obtained.

A few observations with the plate in the other position, *i.e.*, with the axis of the cylinder parallel to the long arm of the whirling machine, have been made. They are given in Tables IV and V.

In Tables I, II, IV, and V the results have been reduced to pressures per sq. ft.; this has been done by multiplying by 12/11·7 and 12/9 respectively. In Table III, the rectangle contained by the two straight edges and the chords of the curved edges contains 1 sq. ft. (the length being 28·8 in.); hence in this case no reduction is necessary.

Table I.—Axis of Cylinder inclined to the Wind.

Fig. 1.



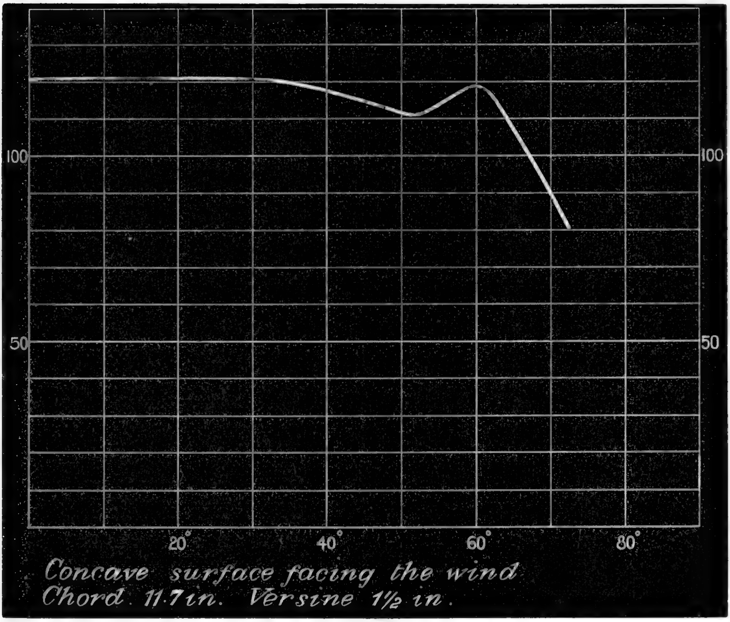
Concave surface facing the wind.

Position I. Angle of incidence.	Value of moment.	Position II. Angle of incidence.	Value of moment.
0°	135, 121, 116, 117		
20	113, 118	20°	136, 131, 120, 117
40	108	30	129, 121, 107, 114, 110
45	111	40	128, 130
50	107, 109	45	135, 131, 113, 113
60	108	50	139, 120, 113, 123, 108
*70	*89	55	138, 125, 127
*80	*59	60	126, 130, 134, 138
		70	96, 100, 96, 98

Convex surface facing the wind.

0	......	87, 77, 76			
20	......	78, 82, 86	20	....	72, 73, 82, 87
40	......	73, 75, 71, 71	40	....	63, 64
50	......	69, 67, 65	45	....	78
60	......	62, 65, 59	50	....	65
70	......	*74, *71	60	....	63, 55
			70	....	56, 58, 60

Normal Component. From Table I.



Normal Component. From Table I.

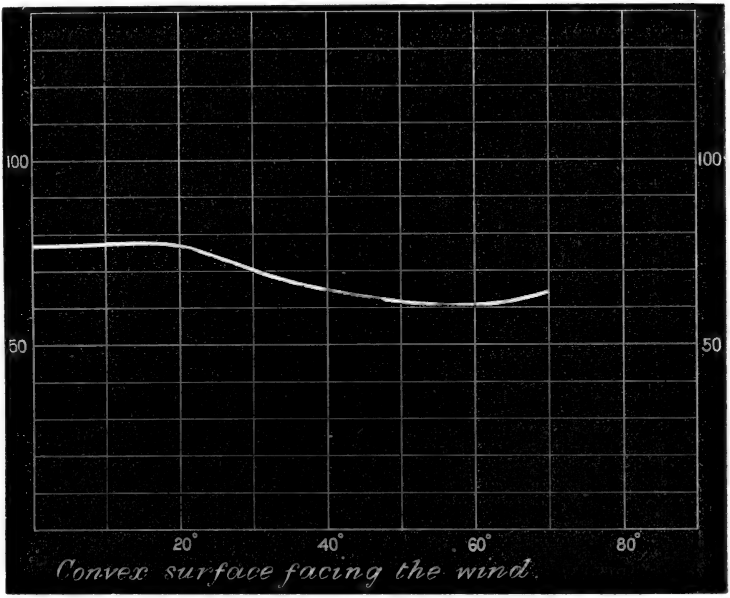


Table II.—Axis of Cylinder inclined to the Wind.

Fig. 2.

Section of cylinder.

Concave surface facing the wind.

Position I.

Angle of incidence.	Value of moment.
0°	127, 129, 133, 125
20	124, 136
40	121, 139
50	120, 137
55	*125
60	*105, *99

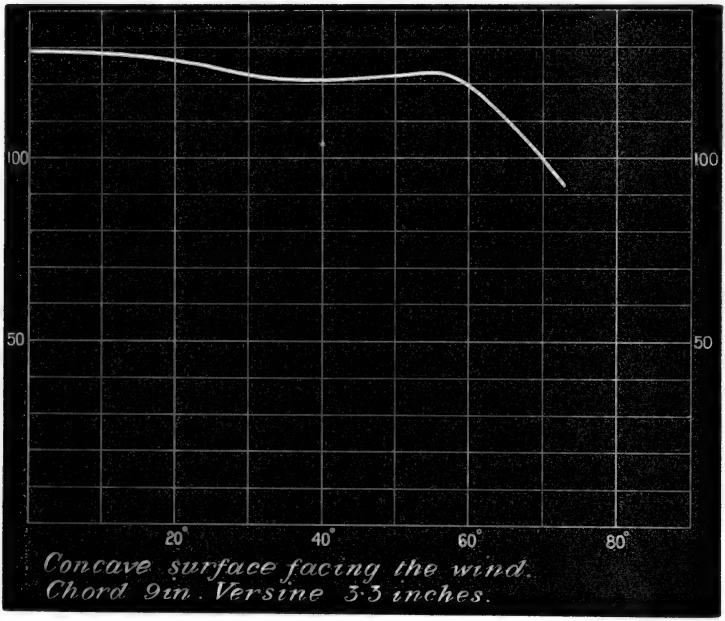
Position II.

Angle of incidence.	Value of moment.
20°	121, 133, 122, 127
40	120, 116
50	121, 119, 116
55	116
60	112, 100

Convex surface facing the wind.

0	60, 63, 65, 68		
20	60, 64	20	60
40	63, 68, 69	40	52, 58
50	61, 64, 75, 73	50	52, 56
60	67, 69, 72, 72	60	47, 53, *56
70	*57, *53	70	*48

Normal Component. From Table II.



Normal Component. From Table II.

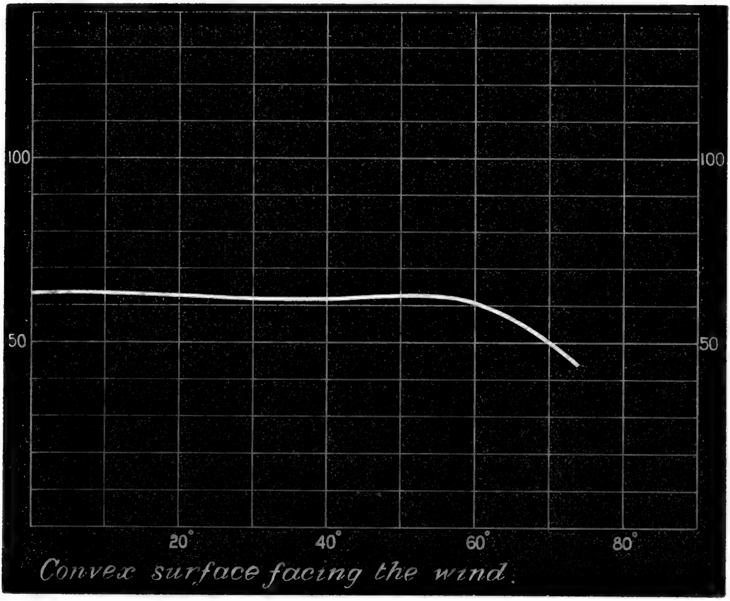


Table III.—Axis of Cylinder inclined to the Wind.

Length of plate, 28·8 in., so that the area of projection of plate might be 1 sq. ft.

Fig. 3.

Section of cylinder.

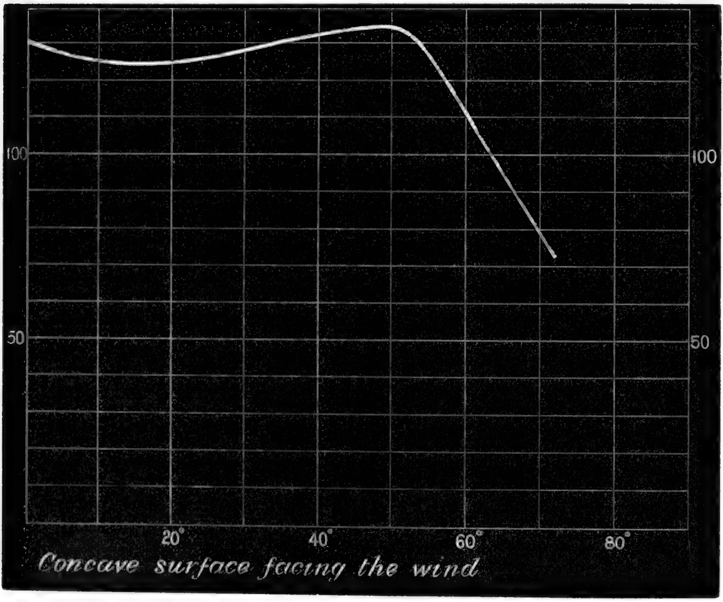
Concave surface facing the wind.

Position I. Angle of incidence.	Value of moment.	Position II. Angle of incidence.	Value of moment.
0°	133, 125		
20	108	20° ..	143, 141, 145, 147
40	101, 103, 105, 105, 106	30 ..	144
50	*90, *87, 83, 88	40 ..	151, 165, *143, *165, 168
60	71, 73	45 ..	*167
70	57	50 ..	*175, 181, 187
		55 ..	*171
		60 ..	143, 148, 152, 157
		70 ..	99, 105

Convex surface facing the wind.

0	....	89, 87, 96, 90	
20	....	89, 94, 96	20 102
40	....	67, 71	40 .... 119, 114, 124
50	....	54	50 .... 131, 130, 145
60	....	42	60 .... 113, 118
70	....	28, 29	70 78
80	....	15	

Normal Component. From Table III.



Normal Component. From Table III.

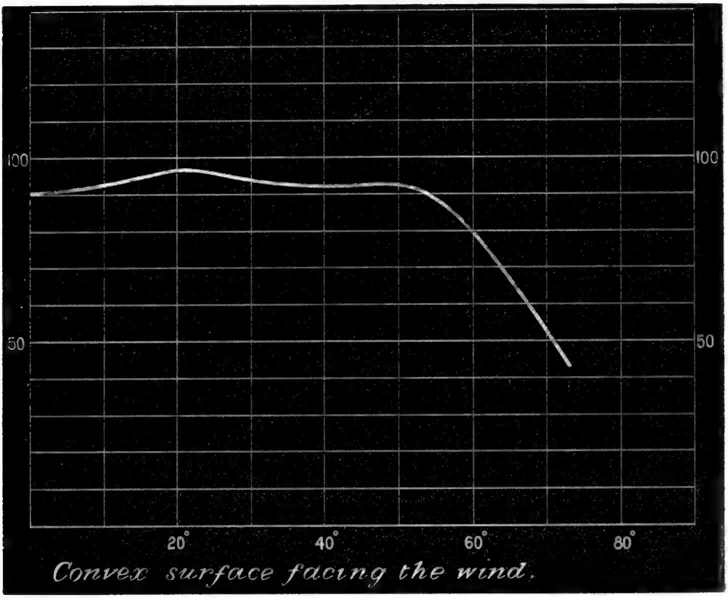
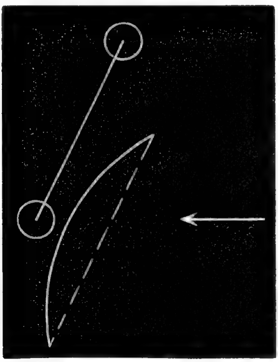


Table IV.—Chord of Cylinder inclined to Wind Direction.

Same plate as Table I. Chord 11·7 in.

FIG. 4.



Concave surface facing the wind.

Position I. Angle of incidence.	Value of moment.	Position II. Angle of incidence.	Value of moment.
0°	111, 116		
$22\frac{1}{2}$	116	$22\frac{1}{2}$°	118
45	*124, 130	45	117, *113

Convex surface facing the wind.

0	86		
$22\frac{1}{2}$	65	$22\frac{1}{2}$	76
45	43	45	46

Table V.—Chord of Cylinder inclined to the Wind.

Same plate as in Table II. Chord 9 in.

Concave surface facing the wind.

Position I. Angle of incidence.	Value of moment.	Position II. Angle of incidence.	Value of moment.
0°	131		
20	*152	20°	120, 130
		40	134

Convex surface facing the wind.

0	64		
20	45, 46	20	56
40	24	40	24

Experiments for the purpose of finding how the curvature influences the resistance at perpendicular incidence have also been made.

The curvature of the plate was gradually increased by drawing the opposite edges more closely together, and the corresponding pressures were obtained, both with the concave and convex surfaces facing the wind.

The projection of the plate upon a plane perpendicular to the wind direction becomes less as the curvature increases, but the pressures have been reduced to unit area, so that they may be easily comparable. It should be noted, however, that the pressure upon a rectangle is less than upon an equal square, the difference being considerable if the rectangle be long and narrow.

The results are given in the following table :—

Table showing the Relation between Resistance and Curvature.

The negative sign placed before the versine means that the convex surface is facing the wind. In each case the area of the plate is 1 sq. ft.

Chord.	Versine.	Area of projection.	Relative pressure per sq. ft.
7·6 in.	−3·8 in.	0·633 sq. ft.	72
8·4 „	−3·6 „	0·70 „	72
9·0 „	−3·3 „	0·75 „	70
10·8 „	−2·1 „	0·90 „	80
11·6 „	−1·5 „	0·97 „	82
12·0 „	0 „	1·00 „	114
12·0 „	0·5 „	1·00 „	126
11·8 „	1·0 „	0·98 „	129
11·6 „	1·5 „	0·97 „	130
10·9 „	2·0 „	0·91 „	127
9·0 „	3·3 „	0·75 „	129

The values could not be obtained beyond this on account of the fluttering of the plate.

The following values are given here for the sake of comparison. They were obtained in May, 1889, by a similar method. The pressures are expressed per sq. ft. in the same scale :—

A 9-in. Robinson cup, concave	132
„ convex	45
A 5-in. Robinson cup, concave	126
„ convex	55
A plate 6 in. diameter, with cone angle 90° at back ..	112
The same with cone in front	74
A plate 6 in. diameter, with cone angle 30° at back ..	115
The same with cone in front	45

From these values the curve given below showing the relation between the pressure per unit area and the curvature has been constructed in a manner suggested by Professor Darwin. The ordinates give the resistance per unit area of projection of plate, and the abscissæ the angle subtended by a section of the plate at the centre of curvature.

There are one or two points in the curve which call for special notice.

The scale of pressure is the same as in the other tables and diagrams, and 100 in the scale represents a pressure of 1 lb. per sq. ft. at a velocity of $18\frac{1}{2}$ miles per hour. It was originally chosen so that 100 might denote the pressure upon a square plate of

1 sq. ft. area exposed normally, and hence the curve should intersect the middle line at a point where the ordinate is 100. It does not do so, however, partly on account of the eddy from the frame (see preceding paper), and partly because the two days on which the experiments relating to this curve were made both happened to be days on which the pressure was above the average.

The slight turning up of the line near the two ends may, perhaps, be due to the smaller area of projection and consequent increase of pressure per unit area in those positions, or it may be due to errors of observation. The curve was obtained from experiments upon one plate only, and it is not unlikely that a slightly different form might have resulted from the use of a larger or smaller square plate.

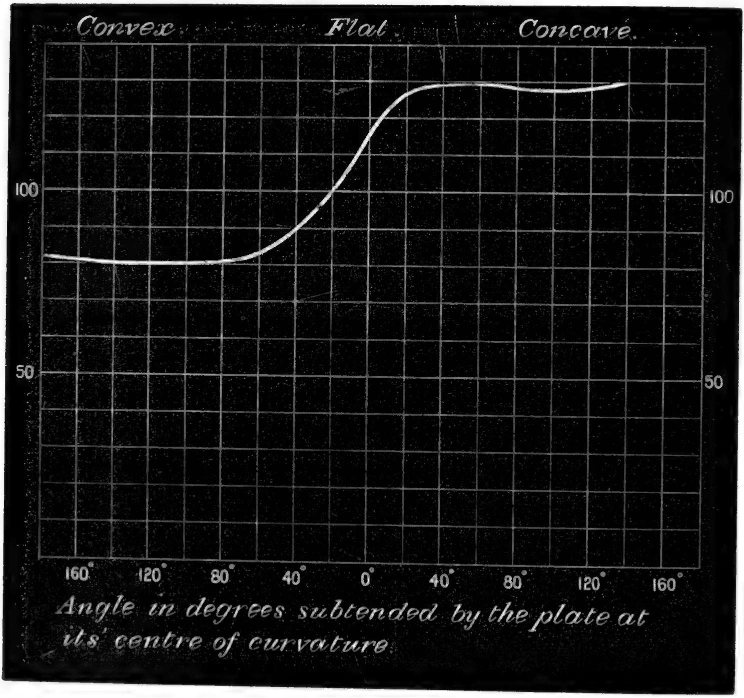


Diagram showing the Relation between the Resistance of a Curved Plate at Perpendicular Incidence per Unit Area of Projection and the Curvature.

IV. "Quadrant Electrometers." By W. E. AYRTON, F.R.S., J. PERRY, F.R.S., and W. E. SUMPNER, D.Sc. Received May 19, 1891.

(Abstract.)

In 1886 it was noticed, on continuously charging up the needle of Sir William Thomson's bifilar suspension quadrant electrometer No. 5, made by Messrs. White, of Glasgow, and in use at the laboratories at the Central Institution, that the deflection of the needle, when the same P.D. (potential difference) was maintained between the quadrants instead of steadily increasing, first increased, and then diminished; so that, both for a large charge on the needle as well as for a small, the sensibility of the instrument was small. A similar effect had been described by Dr. J. Hopkinson, in the 'Proceedings of the Physical Society,' vol. 7, Part 1, for the previous year, and the explanation he gives of this curious result is, that if the aluminium needle be below the centre of the quadrants, the downward attraction of the needle which varies with the square of the needle's charge increases the pull on the bifilar suspension, and so for high charges more than compensates for the increased deflecting couple due to electrical action. On raising, however, the needle of our electrometer much above the centre of the quadrants, the anomalous variation of sensibility of the instrument with increase of charge in the needle did not disappear, and even when the needle was raised so that it was very close to the top of the quadrants, and when, if Dr. Hopkinson's explanation were correct, the sensibility (or deflection corresponding with a given P.D. between the quadrants) ought to have been very great for a large charge on the needle, it was, on the contrary, found to be small.

The needle was carefully weighed, with the platinum wire attached and the weight dipping into the acid, and a calculation was made as to the magnitude of the effect that should arise from the change of the pull of the fibres due to any upward or downward attraction of the needle by the quadrants. This calculation showed that for a P.D. of 3000 volts between the needle and the quadrants, the amount of such attraction was quite unable to account for the observed diminution of sensibility with large charges in the needle. Dr. Hopkinson says in his paper, "Increased tension of the fibres from electrical attraction does not therefore account for the whole of the facts, although it does play the principal part." The experiments that we made at the end of 1886 and beginning of 1887, confirmed by the calculation above referred to, proved that, at any rate in our specimen of the quadrant electrometer, the principal part of the anomalous

action was not caused by an increased tension of the fibres, and that therefore some other cause must be looked for to explain the observed results.

An investigation, which turned out to be both lengthy and very laborious, was therefore undertaken to ascertain the cause of this curious behaviour of our White electrometer. At first we thought that it might be due to some capillary action between the platinum weight and the sulphuric acid, which varied with the potential of the acid, but, experiment having shown that this was not the explanation, we decided to make an exhaustive series of experiments for determining the laws connecting the variation of sensibility of the White quadrant electrometer with the potential of the needle, the distance between the silk fibres, and the distance between the quadrants. The investigation has occupied us on and off for some years, and in carrying it out the quadrant electrometer has had to be taken to pieces many times.

To facilitate the frequent removal of the interior of the Leyden jar, rendered necessary for carrying out the various experiments, an improvement was introduced into the method of clamping the needle, and to diminish leakage, an improvement was introduced into the replenisher, both of which are described in detail in the paper.

The P.D. between the needle and the outside case of the electrometer was measured by means of one of Sir William Thomson's absolute electrometers, made especially sensitive by thinning the coach springs supporting the attracted aluminium disc.

In July, 1888, several large P.Ds. were measured by means of this absolute electrometer (using the constants that we had determined for this instrument), and by means of one of Sir William Thomson's commercial "electrostatic voltmeters," reading to 20,000 volts, kindly lent us by Messrs. Elliott Brothers. The result of these comparisons led first to a correction in the constants that had been previously sent out with the electrostatic voltmeters from Glasgow, and secondly, to a new determination of the value of "v." For Sir William Thomson had calibrated these voltmeters electromagnetically on the basis of the value of the electrochemical equivalent of silver, as determined by Lord Rayleigh and Mrs. Sidgwick, while we had checked the calibration of the electrostatic voltmeter by comparing this instrument with the absolute electrometer. The value of "v" thus obtained was 298 million metres per second.

From the experiments made on varying the distance between the fibres supporting the needle of the quadrant electrometer, it was found that, when the control due to the fibres was large, the sensibility of the quadrant electrometer increased more rapidly than the potential of the needle, whereas, when the control due to the fibres was small, the sensibility increased with the potential of the needle

up to a certain point, and then diminished again as the potential of the needle was still further increased.

From experiments made by varying the distance between the quadrants, we found that when the distance between the quadrants was small, the sensibility first increased as the potential of the needle was raised, then it diminished, and finally it increased again for a still further increase of the potential of the needle. The curve, therefore, connecting sensibility with potential of the needle was invariably of an ∽ shape for a small distance between the quadrants.

As the distance between the quadrants was increased, the sensibility curve flattened, becoming practically straight when the distance separating the quadrants was 3·9 mm. For a greater distance than this between the quadrants, the sensibility increased more rapidly than the potential of the needle.

The various curves accompanying the paper show that this quadrant electrometer may be adjusted so that the variation of sensibility with the potential of the needle may be made to follow one or other of *three distinct laws.* If the quadrants be near together, there are certain limits between which the potential of the needle may vary without producing more than a small change in the deflection corresponding with a fixed P.D. between the quadrants; for example, when the quadrants were about $2\frac{1}{2}$ mm. apart, and the fibres near together at the top, the deflection produced by a P.D. of 1·45 volts between the quadrants only varied about 11 per cent. when the potential of the needle varied from 896 to 3586 volts, that is, by 2690 volts. When the fibres were far apart at the top, it was when the quadrants were about 1 mm. apart, as seen in sheet III, that a similar flatness was obtained in the curve connecting deflection with potential of the needle. In this case the deflection of the needle was practically quite constant when its potential varied between 2152 and 3227 volts, and even when the potential of the needle was increased from 1434 to 3407 volts, that is, by nearly 2000 volts, the deflection did not increase by as much as 9 per cent. This arrangement of the quadrants gives but a comparatively small sensibility, but, where great sensibility is not required, it would be a convenient one to employ, as leakage of the Leyden jar, or loss of potential of the needle due to the rapid absorption that occurs when the jar is first charged, would only slightly affect the deflection for a fixed P.D. between the quadrants.

When the quadrants were at about 3·9 mm. apart, the deflection for a given P.D. between the quadrants was almost directly proportional to the potential of the needle. This then would be the arrangement to employ when the electrometer is used with alternating P.Ds. And lastly, when the quadrants were 4 mm. or more apart, the deflection increased much more rapidly than the potential of the

needle, so that maximum sensibility, bordering on instability, is obtained with this arrangement of the quadrants.

After carrying out a large number of experiments, the cause of the irregularity in the action of the Thomson quadrant electrometer, as made by Messrs. White, began to dawn on us. The wire supporting the aluminium needle, as well as the wire which connects the needle with the sulphuric acid in the Leyden jar, is enclosed in a metallic guard tube to screen the wire from external action. But, in order that the needle may project outside the guard tube, openings are made in its two sides. Hence the moment the needle is deflected from its zero position, each half of the needle becomes unsymmetrically placed relatively to the two metallic pieces which join the upper and lower half of the guard tube. Therefore, in spite of the needle and the guard tube being always maintained at the same potential, there is a repulsion between the charges on the two connecting pieces of the guard tube and the charges on the two halves of the needle. And this repulsion has not only the defect of seriously diminishing the sensibility of the quadrant electrometer as made by Messrs. White, but causes the variation of sensibility of the electrometer with variation of the P.D. between the needle and the outer coating of the Leyden jar to follow a far more complicated law than that expressed by the conventional formula just given.

To test this theory, that the peculiarities in the law of the quadrant electrometer are due to the electric action of the guard tube on the needle in consequence of the special shape of the former, we intensified and varied the want of symmetry of the guard tube by attaching a piece of thin aluminium foil to it above and below the needle, and experiments made on the law connecting the sensibility of the electrometer with the potential of the needle showed that the law could be much altered in character by a slight shift in the position of the piece of aluminium foil.

The paper then goes on to describe experiments connecting the motion of the electrical zero with the potential of the needle, and with the position of the adjustable quadrant.

Guided by the results of a long course of experiments on the White electrometer, we were led, with the assistance of Mr. Mather, to construct an improved unifilar quadrant electrometer which is fully described and illustrated in the paper. This improved electrometer differs in numerous particulars from that made by Messrs. White. The bifilar suspension is abandoned for reasons given in the paper, and there is employed instead a new form of adjustable magnetic control, so arranged that the needle is practically unaffected by outside magnetic disturbance. All the working parts are supported from the base, so that on removing the glass shade, which serves as the Leyden jar, all the parts can be got at and adjusted *in position;* all the

insulated stems are made of glass, and are under cover, protected from dust and damp; pressure contact between the electrodes and the quadrants is replaced by spirals of fine wire screwed to the quadrants and to the electrodes; the needle, quadrants, and guard tube are so shaped that, in whatever symmetrical position the quadrants be placed, the deflection produced by a given P.D. between the quadrants is directly proportional to the potential of the needle, and further, this improved electrometer is at least ten times as sensitive as our specimen of the White pattern when the instruments are adjusted to be in equally trustworthy condition as regards definiteness of the zero and of the deflected position of the spot of light.

Next follows an account of some experiments made by us on a White electrometer, the needle of which Mr. Boys had suspended with a single quartz fibre. Although this instrument was in excellent condition as regards definiteness, &c., the raising of the potential of the needle to only 400 volts was sufficient to show that the sensibility was not proportional to the needle's potential.

Lastly, for the purpose of obtaining conclusive evidence as to whether our idea was correct about the connecting pieces of the guard tube in the White electrometer causing the sensibility of this instrument to be in many cases actually less when the needle had a high potential than when it had a low, we had constructed a little collar with two legs. This collar could be clamped to the upper portion of the guard tube of the improved electrometer with the legs projecting down into the quadrants on each side of the needle, and experiments showed that when this collar was attached to the guard tube the improved electrometer, although not a bifilar instrument, became as bad as the White pattern. For while before the attachment of this collar the sensibility increased proportionately to the potential of the needle, after the collar was attached the sensibility first increased and then diminished again as the potential of the needle increased, and with the same adjustment of the quadrants, controlling magnets, &c., and with the needle charged to a potential of 1300 volts, the mere attachment of this little collar reduced the sensibility to one quarter.

The paper concludes with a sketch of the mathematical investigation that we carried out, and it is explained that by taking into account the electrical action of the connecting pieces of the guard tube of the White electrometer, the diminution in this action as the quadrants are pulled out, the alteration produced by the tilting of the needle at high potentials on the magnitude of this electrical action as well as on the rate of variation, per radian deflection of the needle, of the coefficient of induction between the insulated pair of quadrants and the needle, an expression was obtained for the deflection of the needle in terms of its potential and the P.D. between the

quadrants. And this expression, although containing only three constants, fitted with considerable accuracy all the curves given in the several sheets accompanying the paper.

The results of the investigation, briefly summed up, are as follows:—

1. The quadrant electrometer as made by Messrs. White, although it may be carefully adjusted for symmetry, does not usually even approximately obey the recognised law for a quadrant electrometer when the potential of the needle is altered.

2. The peculiarities in the behaviour of the White electrometer are due mainly to the electrical action between the guard tube and the needle, and to the slight tilting of the needle that occurs at high potentials.

3. By special adjustments of the quadrants of the White electrometer the sensibility can be made to be either nearly independent of the potential of the needle, or to be directly proportional to the potential, or to increase more rapidly than the potential of the needle.

4. By altering the construction of the instrument as described, the conventional law for the quadrant electrometer is obtained without any special adjustment of the quadrants beyond that for symmetry, and the instrument is rendered many times as sensitive as the specimen we possess of the White pattern.

V. "Researches on the Absorption of Oxygen and Formation of Carbonic Acid in ordinary Human Respiration, and in the Respiration of Air containing an Excess of Carbonic Acid." By WILLIAM MARCET, M.D., F.R.S. Received May 25, 1891.

Allow me to begin by recording the valuable help I have experienced throughout the present enquiry from my assistant, Mr. Edward Russell, F.C.S. We have both put our shoulders to the wheel, and have gone together through the great number of calculations the work entailed. I am much indebted to Mr. Russell for the pains he has taken, and the accuracy of his judgment whenever a knotty point had to be met and overcome.

My object in the following paper is to give an account of the consumption of oxygen in human respiration, or, in other words, to determine the proportions of oxygen transformed into carbonic acid, and of oxygen retained in the blood, to which is added a short inquiry into the effects produced by the inhalation of air containing CO_2 on the interchange of the pulmonary gases. The investigation was carried out, so far, on myself and Mr. Russell, while under the

influence of food and when fasting, or at a period of four hours at least from breakfast, when a desire for lunch was clearly felt.

The results aimed at, although applying to man instead of animals, were similar in kind to those obtained on animals by Messrs. Regnault and Reiset,* and more recently by Messrs. Chapman and Brubacker, of Philadelphia.† Regnault and Reiset by their admirable researches have paved the way to a correct history of the chemical phenomena of respiration; and Chapman and Brubacker, who have repeated these experiments by a similar method, deserve much praise for their laborious and interesting investigation, confirming, in a marked degree, the results obtained by Regnault and Reiset.

Rabbits being the subject of the experiments, Regnault and Reiset obtained for the relation between the oxygen consumed and CO_2 produced a mean figure of 0·919 from six experiments; with Messrs. Chapman and Brubacker the corresponding result, also from rabbits, was 0·90.

These experiments were made by confining animals in a receiver or bell-jar, and absorbing the CO_2 they produced with potassium hydrate aided by mechanical means, while oxygen was supplied automatically as fast as the CO_2 was absorbed, and as nearly as possible in equal volumes. An examination of the figures expressing the results obtained by Regnault and Reiset and the American physiologists will show that the animals at the end of the experiment had to breathe an atmosphere containing an excess of CO_2, as it was impossible to rid the air entirely of this gas. The proportion of CO_2 present in the air at the close of the experiment occasionally rose to 3 per cent. and higher, and this is a rather large contamination to allow of results being applied to natural breathing, considering that atmospheric air contains only from 0·04 to 0·1 per cent. CO_2.

Moreover, the proportions of oxygen in the chamber at the end of the experiments varied considerably, although always lower than the corresponding proportion in atmospheric air. So that, towards the end of the experiments, the animals were breathing air containing an excess of CO_2 and a deficiency of O.

There must be another difficulty to contend with in such kinds of experiments, amounting to the impossibility of keeping the animals quiet, and muscular action exerts, we know, a very positive influence on the phenomena of respiration.

In addition to the labours of Regnault and Reiset, and Chapman and Brubacker, I have to quote the papers of Carl Speck,‡ and of

* 'Annales de Chimie et de Physique,' 1849.

† 'Proceedings of the Academy of Natural Sciences of Philadelphia,' January, 1891.

‡ "Experimentelle Untersuchungen über den Einfluss der Nahrung auf Sauerstoffverbrauch und Kohlensäureausscheidung des Menschen," von Carl Speck, 'Archiv für Pathologie,' 1874.

Messrs. Jolyet, C. T. Bergonié, and Sigalas,* while an elaborate paper has appeared last month (April) in the 'Annales de Chimie et de Physique,'' by Messrs. Hanriot and Ch. Richet, which also treats of the interchange of the respiratory gases in man. In the experiments last mentioned, the air inspired was breathed through a gas meter, and then expired through another meter. Next, the air expired was conducted through an apparatus destined to the absorption of the CO_2, and finally through a third meter. The meter on the inspiratory track showed the volume of air inspired, the first expiratory meter registered the volume of air expired, and the second the volume of CO_2 produced, which was equal to the difference of volumes as indicated by the two meters. The difference of volume registered by the inspiratory and first expiratory meters yielded the volume of oxygen absorbed.

The method is ingenious; at first sight it appears satisfactory, but on looking into the process with an experience acquired from about 15 years' work, on and off, on the chemical phenomena of respiration, and with the knowledge of the difficulties concerning the volumetric determination of carbonic acid, I cannot help considering the method too rough for an inquiry which requires extremely delicate manipulation.

I must also take exception to the use of the face-pieces and valves which were introduced in these experiments, though reluctantly, as the authors remark. In my earliest inquiries on the chemical phenomena of respiration, face-pieces and valves were employed; but eventually I gave them up from their interfering with free respiration, and from the difficulty of maintaining the valves in an absolutely reliable state.

Method of Investigation and Instruments.

The method of investigation adopted in the present researches is quite different from any of those made use of by other authors. Every care was taken to breathe naturally during the experiment. The recumbent position was assumed in a deck chair, with the body perfectly supported, and the person under experiment inspired through the nose and expired through the mouth, compressing his nostrils, during expiration, if it was thought necessary, with a slight motion of the hand, or of the index fingers of both hands. The movement was, indeed, hardly perceptible, and could not, by any means, influence the CO_2 expired. After a sufficient period of rest had been allowed, the expired air was collected in a receiver or bell-jar suspended over salt-water, which has been described on former occasions. The re-

* "Échanges gazeux pulmonaires dans la respiration de l'homme," 'Comptes Rendus,' 1887.

ceiver was so carefully counterpoised that the person under experiment could not tell whether he was breathing into it or into the external air. It was supplied with a scale divided into litres and fractions of litres; an oil gauge showed the pressure of the air within it, and a thermometer its temperature. A number of precautions were taken in connexion with the mode of collecting the air expired, which cannot be entered into at present. I have satisfied myself that breathing into these bell-jars is identical with natural respiration: the volumes expired, say, per minute really corresponding with the volume of air expired per minute while breathing naturally into the open air. Each experiment lasted between 7 and 8 minutes.

The determination of carbonic acid in the air expired was made exactly in the way described in a previous paper to the Royal Society.* The air expired was transferred from the bell-jar to a cylinder, shaken with baryta-water, and finally determined by titration, according to Pettenkofer's method.

The oxygen was determined in a eudiometer, constructed on the same principle as the eudiometer I have described in the 'Proceedings of the Royal Society,'† but modified and improved. Instead of a straight tube this instrument consists of a U-tube, with a neck and a glass stop-cock at its bend. One limb is left open, and the other is closed at the top by an iron cap, in which a three-way cock is fitted, perfectly air-tight. Two short iron tubes project beyond the tap. Both limbs of the U-tube are graduated. The limb bearing the iron cap is graduated into cubic centimetres, and the open limb is graduated into divisions corresponding exactly with those on the other limb, so that, whatever be the level of the mercury in the U-tube under atmospheric pressure, the readings are identical in both limbs. The scale at the back of the open limb is movable, so that it can be brought easily into its proper position and fixed there. The closed limb is surrounded with a water jacket, and the iron cap is partly immersed in water, while the platinum wires are embedded in a shellac cement, so as to be effectually protected from contact with the water.

The hydrogen for exploding the gas was prepared with every care from zinc and sulphuric acid; it was washed first through a strong solution of potassium hydrate, and then through water. A volume of hydrogen, at least nine or ten times that of the air-spaces in the Woulffe's bottles, was passed through before collecting the gas; it was finally aspired into a glass bell-jar of a capacity of about a litre, and movable up and down in a glass receiver holding water.

* "A Chemical Enquiry into the Phenomena of Human Respiration," 'Phil. Trans.,' B, 1890.

† "A new form of Eudiometer," 'Roy. Soc. Proc.,' June, 1888.

Before the hydrogen was used it was tested in nearly every case with atmospheric air.

The determinations of oxygen were made as follows :—The U-tube was first of all filled to overflowing with mercury through its open limb, the three-way cock being turned so as to let out the mercury after filling the tube; by this means every trace of gas was driven out of the tube.

Then the stop-cock was turned so as to close the eudiometer and let the hydrogen gas through it, and the instrument having been brought into connexion with the gas-holder by india-rubber tubing, the hydrogen, under a pressure of an inch or more of water, was driven rapidly through the stop-cock, being, moreover, aspired by the dilatation of an india-rubber syringe. The cock was now turned so as to admit the hydrogen into the eudiometer, and the mercury, being let out at the bend of the U-tube, aspired the required volume of hydrogen into the instrument; this amounted to from 18 to 20 c.c. The bell-jar was then placed under atmospheric pressure, the gas turned off, and the height of the mercury if not exactly the same in both limbs was adjusted by adding or withdrawing mercury until the readings were alike.

The air to be analysed for the determination of oxygen had been collected by displacement with water in a cylinder holding from 1 to 1·5 litre, and shaken with a solution of barium hydrate, to rid it entirely of its carbonic acid. The cylinder was now placed on a stand, over which was disposed a glass receiver full of water, communicating by india-rubber tubing with the lower end of the cylinder. The india-rubber tube was carefully filled with water, so as to let no air into the cylinder, and then the cylinder was placed in communication with the eudiometer. The next stage was to wash out the passage in the stop-cock with the air to be analysed; this was done by connecting by india-rubber tubing the three-way cock with the cylinder, and some 200 or 300 c.c. of air were driven through it from the cylinder by the pressure of the water in the receiver; the iron tube was now stoppered on the opposite side by a short rubber-tube and pinch-cock. The mercury in the eudiometer was next let out at the bottom until a sufficient diminution of pressure had been obtained to aspire the requisite volume of air from the cylinder. This air was admitted from the cylinder while under pressure, and taken into the eudiometer by aspiration, so that the effect produced was that of a piston driving the hydrogen before it, and giving it no time to diffuse out of the instrument. The height of the mercury was now adjusted in both limbs by pouring in mercury, or letting it out at the bottom, and finally the reading was taken and recorded. The air and hydrogen were now thoroughly mixed by fitting an india-rubber syringe to the open end of the eudiometer and pressing it with the hand; a

succession of pressures soon effected a perfect mixture. The mixture was exploded as usual with a battery, when but little commotion was produced. Then mercury was added through the open limb, and the level adjusted in the two limbs; a very short time sufficed to ensure no further contraction or dilatation, and then the height of the mercury was read off. In the calculation of the analyses a slight correction was introduced, from the increased temperature of the water in the jacket owing to the flash.

This method proved extremely convenient and reliable. An experiment could be commenced in the afternoon, say 4 o'clock; the combination with barium hydrate for the determination of CO_2 was effected immediately after the air had been expired, and the turbid fluid left till the following morning for titration. Next, air was drawn into another cylinder by displacement with water, shaken with a solution of barium hydrate for about 12 minutes, and then the cylinder was placed on the stand for the determination of the oxygen in the air it contained. Fresh hydrogen was prepared for each experiment, and, as stated before, it was, in nearly every case, tested with atmospheric air before being used.

The calculations of the analyses were made as follows: the volume of the air expired, amounting to about 36 litres, was reduced to the dry state, 0° C., and 760 mm. pressure. This was done very rapidly by means of the Table I have constructed for the purpose.* Then the volume of CO_2 present was easily obtained from the weight of this gas found in the analysis. Next, the volume of CO_2 was subtracted from the volume of air expired, and the oxygen calculated on the reduced volume. With these data concerning the volume of air expired, a Table was constructed, of which the following is an illustration :—

Table Illustrating the Results of an Experiment.

Volume air inspired.			Volume air expired.		
	33259	100		33008	100
CO_2....	30 c.c..........	0·09	CO_2....	1885 c.c.	5·71
O.......	6961 „	20·93	O.......	4855 „	14·71
N	26268 „	78·98	N	26268 „	79·58
	33259	100		33008	100

Volume oxygen consumed..........................	2106	c.c.
Volume carbonic acid produced	1855	„
Ratio of oxygen consumed to CO_2 produced...........	0·881	
Volume oxygen absorbed..........................	251	c.c.

* 'Phil. Trans.,' 1890.

Volume oxygen absorbed per minute.................	39·6	c.c.
Volume oxygen absorbed per 100 air inspired.........	0·75	,,
Volume oxygen absorbed per 100 O inspired	3·61	,,
Weight oxygen consumed per hour..................	28·60	grams.
Weight oxygen consumed per kilo. weight of body	0·416	,,
Weight CO_2 expired per minute....................	0·577	,,

The volume of air *inspired* was calculated from the *nitrogen* found in the air expired, and this is one of the main features of the present paper. A number of experiments were undertaken to try if any accurate determination could be made of the air inspired and expired by filling a counterpoise bell-jar with a measured volume of air, inspiring this air through the nose and expiring it into an empty bell-jar through the mouth. The plan, however, did not prove successful, and it was found impossible by this means to determine with a sufficient degree of accuracy the differences of volumes between the air inspired and expired. It then occurred to me that the volume of nitrogen found in air expired might afford a means of determining the volume of air inspired. According to one of the results obtained from Regnault and Reiset's experiments there is, under ordinary circumstances, a trifling amount of nitrogen exhaled from the blood in the process of respiration. The volume of this gas is, however, so small that its mean proportion in dogs fed with meat was only found to amount to the 0·0066 part of the oxygen consumed. This means from 11 to 13 c.c. in 1800 or 2000 c.c. of oxygen consumed and in about 33,000 c.c. of air breathed, a figure so low that, practically, the nitrogen exhaled may be ignored in the calculation of the analyses. I have entered the correction in some of the calculations, and it alters the volume of oxygen consumed by about 0·6 per cent., and that of the oxygen absorbed by 1 or 1·5 per cent. These corrections are so small that I have not thought it worth while to make them, and the nitrogen has been taken as the same in the air expired and inspired.

It was now easy to calculate the volume of air inspired. This volume consisted of the atmospheric carbonic acid, oxygen, which was taken in the proportion of 20·93 per cent., and nitrogen. The atmospheric carbonic acid was determined in every experiment by Pettenkofer's method; it ranged from 5 to 10 parts in 10,000. In the course of last April an additional window was made in my laboratory, which allowed of improved ventilation.

Having prepared a table of the constituents per cent. of the air inspired, the *volume* of air inspired was calculated as follows:—The nitrogen (per cent.) in the air *inspired* is to 100, so is the nitrogen in the air *expired* to the volume of air *inspired*. The volume of air inspired is thus obtained with much greater accuracy than by any experimental method, as the nitrogen must invariably exhibit the

same proportion whatever the volume of air expired. By this means the results obtained for the volumes of oxygen *consumed* and *absorbed*, which are the main objects of the present enquiry, are thoroughly reliable.

The experiments are made on two different persons, and show that not only the carbonic acid expired within a given time but also the oxygen consumed varies according to individuals. The subjects of these experiments are well suited to show "extremes" as to the function of respiration, one of them being 63 years of age and the other 21, both in perfect health.

Twelve experiments were made in both cases, six while under the influence of food and six while fasting. No experiments were made under extreme fasting.*

On considering the Tables, the composition per cent. of the air expired is observed to alter but little. In my case the CO_2 varies from 4·53 to 5·14.

The proportion of oxygen was very constant, ranging also with me from 15·30 to 16·0.

The volumes of oxygen consumed represented the volumes of oxygen the body took up, on one hand for the combustion and elimination of carbon in the form of carbonic acid, on the other, for the probable elimination of tissues in the form of crystalloid compounds. The relations between the oxygen consumed and the carbonic acid produced varied in my case between 0·816 and 0·912 with a mean of 0·863.

The proportion of the oxygen consumed which is *absorbed* is easily found by subtracting the volume of CO_2 produced from the total volume of oxygen consumed. This volume has been expressed *as absorbed per minute*, a result obtained by dividing the figure found by the number of minutes and seconds the experiment lasted. The volumes absorbed per minute varied in my case from 21·3 to 42·8 c.c., with a mean of 33·0 c.c.; this was equal to 2·83 grams of oxygen absorbed per hour. It may be concluded that this absorption of oxygen is an important factor towards the phenomena of nutrition.

The proportion of oxygen *absorbed* in my case for 100 parts of *air* inspired exhibits a mean of 0·74 part, and varies from 0·44 to 1·03. This does not agree with the volume of oxygen usually considered as absorbed, amounting to about 2 per cent. The corresponding proportion for the experiments on Mr. Russell will be found nearly exactly that obtained for myself, and I must conclude that 0·74 per cent., or a closely approximating figure, shows the proportion of the air inspired (in the form of oxygen) which remains in the blood, and consequently does not reappear in the corresponding air

* See Tables accompanying this paper.

expired. The mean proportion of oxygen absorbed in the *oxygen* inspired amounted to 3·57 per cent.

The weight of oxygen consumed per hour varies according to the person under experiment, and also in relation to the lapse of time after the ingestion of food. It ranged in my case from 19·75 grams to 22·02; and per kilo. weight of my body, from 0·338 to 0·376 gram. This is a much smaller proportion than that given for animals either by Regnault and Reiset or Chapman and Brubacker.

Influence of Food.—Food, in my case, at a mean time of 2 hours and 16 minutes after its ingestion, exerted apparently no effect on the proportions of CO_2, O, and N expired, as they were nearly exactly the same in both cases.

The ratio between the oxygen consumed and CO_2 produced exhibited, in my case, a decided tendency to fall while under the fasting state, the figures obtained being 0·870 while under the influence of food, and 0·850 while fasting.

The volumes of oxygen absorbed were much the same in both cases, although exhibiting a tendency to rise while fasting.

The differences between the proportions of oxygen absorbed in 100 volumes of air, or 100 of oxygen, under the influence of digestion or fasting are inappreciable.

There is a decided excess in the weight of oxygen consumed under the influence of food over that consumed fasting; the figures being 21·37 grams after food, and 20·26 grams fasting.

The weight of CO_2 expired per minute varies as usual according to the influence of food, and calls for no comment.

Mr. Russell also submitted to twelve experiments, six made at a mean time of two hours after a meal, and six while fasting, or at a mean time of four hours and twenty-three minutes after food. The proportions per cent. of CO_2, O, and N expired are much alike in every experiment, varying as follows:—CO_2, 5·38 to 5·96; O, 14·39 to 15·26; N, 79·36 to 79·92. The ratio of oxygen consumed to CO_2 produced varies from 0·818 to 0·923, with a mean of 0·878. This closely approximates the corresponding means obtained in my own case, amounting to 0·863, which, however, is slightly lower. The next figures in the table, showing the proportion of oxygen consumed on 100 of air and 100 of O breathed, give means very nearly the same as when I submitted to experiment.

The weights of oxygen consumed per hour, 25·98 grams, and per kilo. of body weight per hour, 0·380, are decidedly higher than in my case. Mr. Russell also expired a greater weight of CO_2 per minute than I did, showing greater activity in the process of nutrition—a phenomenon probably due to youth.

The influence of *digestion* and *fasting* shows no alteration in the proportions of CO_2, O, and N expired. The difference in the ratio

between O consumed and CO_2 produced is barely perceptible, although exhibiting a slight tendency to fall while fasting. The proportions of oxygen absorbed in 100 of air and oxygen inspired are much the same under the influence of food and fasting.

The main effect produced on Mr. Russell by the ingestion of food and fasting is to be found in the weight of oxygen consumed per hour, which falls from 28·66 grams under the influence of food to 23·30 when fasting, and per kilo. weight from 0·417 gram to 0·330 gram. There is also, when fasting, a considerable reduction of weight of the CO_2 expired per minute (0·578 gram to 0·468 gram).

The results obtained from the present investigation of the interchange of gases in the respiratory process of man may be summed up as follows:—

1. The percentage of CO_2, O, and N in the air expired alters according to the person under experiment, but in every case the proportions of each gas vary but slightly up to a period of about four hours and a half after the mid-day meal, a result I had formerly obtained for CO_2.
2. The ratio between the oxygen consumed and the carbonic acid produced exhibited a mean of 0·871 for two persons and twenty-four experiments. This is nearly the same figure as that obtained by Messrs. Jolyet, Bergonié, and Sigalas—0·868, and a marked approximation to 0·90, the corresponding ratio given for rabbits by Messrs. Chapman and Brubacker.
3. The mean volume of oxygen *absorbed* per minute was very nearly the same for the two persons, and amounted to a total mean of 34·3 c.c. on twenty-four experiments. This would be equal to 2·94 grams of oxygen absorbed per hour.
4. The mean volume of *oxygen* absorbed in relation to *the air inspired* proved nearly the same in both persons submitted to experiment, and amounted to 0·75 per cent. A similar remark applies to the proportions of oxygen absorbed to the *oxygen inhaled;* the figures are 36·9 in one case, and 3·55 in the other, with a mean of 3·63.
5. The mean weight of oxygen consumed per hour varied with each person submitted to experiment, amounting to 20·81 grams in the older and 26·09 in the younger man. The corresponding figures per kilo. of body weight were 0·355 gram and 0·380 gram.
6. The weight of carbonic acid expired per minute is notably higher in the younger man, and corresponds approximately to proportionally increased amount of oxygen consumed.

The elaborate investigation of Messrs. Hanriot and Richet calls for a few remarks.

The mean ventilation of the lungs, by which expression I conclude Messrs. Hanriot and Richet mean the sum of the air inspired and expired, amounts, according to these authors, to 10 litres of air per kilo. weight of the body per hour. This result agrees, within certain limits, with those I have obtained. The sum of the air inspired and expired in each experiment gives, in my case, a maximum of 9·94 litres per kilo. weight per hour, and a minimum of 8·33 litres, with a mean of 9·12 litres, which is near to Messrs. Hanriot and Richet's figure of 10 litres. Mr. Russell's mean pulmonary ventilation is decidedly less than mine, and lower than the figure obtained by the French authors, amounting to a maximum of 9·51 litres and a minimum of 6·87 litres, the mean being 8·13 litres. It is therefore obvious that the pulmonary ventilation varies per kilo. weight of body, according to different people. Messrs. Hanriot and Richet apparently experimented only on a single person.

The volume of carbonic acid in 100 of expired air appears decidedly low in Messrs. Hanriot and Richet's experiments, amounting to a mean of 3·30. In the experiments which form the subject of the present paper, the corresponding proportion varied, for myself between 4·53 and 5·14, and for Mr. Russell between 5·38 and 5·90. These proportions, to which I have drawn attention in former communications, vary not only with different individuals, but with the same person under different circumstances. The relation between the oxygen consumed and CO_2 produced is decidedly smaller in Messrs. Hanriot and Richet's experiments than in my own; those gentlemen find the mean relation in question to be 0·78, while the mean from my experiments on two different persons yield 0·871.

The second part of the present communication deals with the respiration of air containing from 2·5 to 4 per cent. of carbonic acid. The mixture was made by introducing carbonic acid, prepared from marble and hydrochloric acid, into a certain volume of air drawn into one of the bell-jars. The mixture was first of all analysed for the determination of the CO_2 it contained, and then it was inspired through the nose, by means of a well-fitting nose-piece, and expired into the other bell-jar through the mouth. The first five or six inspirations were used for rinsing out the lungs and the bell-jar, and were driven out of the second bell-jar through a T-piece, while the person under experiment was expiring into the open air; this was easily effected by means of the three-way stop-cock; then the stop-cock was again turned, and the expired air collected in the bell-jar while the time was taken. The effects produced were a sensation of want of air and a considerable increase in the volume of air breathed per minute. The air expired, during a period of from 2 to 4 minutes, was collected for experiment; but the breathing of the air and CO_2 was carried on altogether for 4 or 5 minutes. No lasting ill effects

were produced on either of us. I submitted to three experiments and Mr. Russell to two.

These experiments (with one exception) were undertaken at the same time as others made with fresh air, though about an hour later, and references are entered in the following table in order that the corresponding experiments may be compared with each other.

Air Breathed containing an excess of Carbonic Acid.

Dr. Marcet under experiment.

	Experiment I.	Experiment II.		Experiment III.	
	2·53 per cent. CO_2 inspired. Time after food, $2^h\ 45^m$. Lab. temp., 10°·5. Bar., 758·7.	3·54 per cent. CO_2 inspired. Time after food, $3^h\ 25^m$. Lab. temp., 10°·4. Bar., 756·4.		4·06 per cent. CO_2 inspired. Time after food, $2^h\ 30^m$. Lab. temp., 12°·9. Bar., 762·7.	
	On 100 expired.	On 100 expired.		On 100 expired.	
CO_2	5·88	6·25	Compares with Experiment No. 5 on ordinary respiration (same sitting).	6·29	Compares with Experiment No. 6 on ordinary respiration (same sitting).
O	16·14	16·21		16·78	
N	77·98	77·56		76·93	
O consumed ..	858	1085		831	
CO_2 produced.	633	668		505	
Relation......	0·738	0·616		0·608	
O absorbed per minute......	71	112		127	
O absorbed on 100 air inspired	1·16	1·63		1·39	
O absorbed on 100 O inspired	5·71	8·08		6·90	
Weight O consumed per hour	23·31	25·11		27·84	
Weight O per hour per kilo.	0·398	0·429		0·476	
Weight CO_2 expired per minute......	0·393	0·354		0·388	

Mr. Russell under experiment.

	Experiment I.		Experiment II.	
CO_2 in air breathed	3·79 per cent.		3·91 per cent.	
Time after food	3 hours	Compares with Experiment No. 4 on ordinary respiration (same sitting).	3 hours	Compares with Experiment No. 6 on ordinary respiration (same sitting).
Laboratory temperature	10°·9		16·7	
Barometer	759·5		764·0	
On 100 parts air expired.				
CO_2	6·62		6·54	
O	15·90		15·80	
N	77·48		77·66	
O consumed	1073		1079	
CO_2 formed	642		579	
Relation	0·598		0·537	
O absorbed per minute	141 c.c.		170 c.c.	
O absorbed per 100 air expired	1·82		2·15	
O absorbed per 100 O expired	9·03		10·70	
Weight O consumed per hour	30·26		31·64	
Weight O consumed per kilo	0·440		0·460	
CO_2 expired per minute	0·415		0·389	

On a consideration of the foregoing tables it will be seen that the CO_2 expired (per cent. of air expired) does not represent the CO_2 exhaled from the blood, but the figure is much higher, as it includes the CO_2 inspired, which is expired together with the proportion exhaled. The volume of CO_2 actually exhaled from the blood can be calculated by subtracting the proportion of CO_2 in the air *inspired* from the corresponding proportion of CO_2 in the air *expired.* The volumes of CO_3 actually found in the expired air have not been entered in the following table (p. 71), but the figures represent these volumes less the corresponding proportions of CO_2 in the air inspired, and they show that the CO_2 actually exhaled from the blood is very much less than in ordinary respiration.

It will therefore be observed that nearly half the CO_2 which would have been expired in natural breathing has been retained in the body. Of course this is assuming that no CO_2 has been absorbed directly at the lungs. Hence there must be a very great accumulation of CO_2 in the blood when air containing CO_2 is inspired.

It follows from this inquiry on the respiration of air containing from 2·5 to 4 per cent. of CO_2—

1st. That the proportion of oxygen in 100 of air expired exhibits a slight increase beyond its proportion in ordinary breathing.

2nd. That the relation between the oxygen consumed and CO_2 produced is very much smaller than in ordinary respiration, amounting to a mean of 0·654 for myself and 0·567 for Mr. Russell, against a total mean of 0·871 for both of us in ordinary breathing.

Dr. Marcet.

Percentage CO_2 expired in ordinary breathing.	Percentage CO_2 in air inspired.	Percentage CO_2 exhaled from blood when breathing air and CO_2.
1. ...... 5·12	3·54	2·66
2 ...... 5·12*	2·53	3·32
3. ...... 5·14	4·06	2·17
Means ... 5·13	3·38	2·72
Mr. Russell.		
1. ...... 5·69	3·79	2·76
2. ...... 5·51	3·91	2·55
Means ... 5·60	3·91	2·65

3rd. That the volume of oxygen absorbed per minute is greatly increased, my own mean amounting to 103 c.c., and Mr. Russell's to 155 c.c., instead of 32·2 in my case and 37·5 in the other.

4th. That the proportions per cent. of oxygen absorbed in the air inspired are increased in both cases to a mean of 1·39 against 0·66, and to a mean of 1·98 against 0·75 in ordinary breathing. A corresponding increase is observed in the proportion of oxygen absorbed to the oxygen inspired.

5th. That the weight of oxygen consumed by the body per hour is considerably increased, amounting to a mean in my case of 25·42 grams, against 21·37 grams, and with Mr. Russell of 30·95 grams against 28·66 in ordinary breathing.

6th. That the weight of carbonic acid expired per minute is considerably reduced, amounting in my case to a mean of 0·378 instead of 0·430, and with Mr. Russell to a mean of 0·402 instead of 0·578 expired in normal respiration.

These experiments, although but few in number, suffice to show that when air is breathed containing from 2·5 to 4 per cent. of CO_2 the amount of oxygen consumed is much greater than in ordinary breathing, while the carbonic acid expired is very much less. There must consequently remain in the blood a considerable amount of oxygen to be transformed into an excess of CO_2 besides the proportion required towards the other functions of the body.

* The analyses following fig. 2 do not really correspond, but are made under similar circumstances of food, temperature, &c., and are therefore made to compare with each other in this table.

Experiments on Ordinary Respiration showing the Composition of the Air Expired and the Interchange of Gases.

Experiments made under the Influence of Food.

(Dr. Marcet under experiment.)

	Exp. 1.	Exp. 2.	Exp. 3.	Exp. 4.	Exp. 5.	Exp. 6.	Means after food.
Time of food	2 hours after lunch	2 hours after lunch	3 hours after lunch	2⅓ hours after lunch	2 hours 25 mins. after lunch	1 hour 40 mins. after lunch.	2 hours 16 mins. after lunch.
Laboratory temperature	18·0	17·4	16·2	15·5	10·4	12·0	14·9
Barometer	766·0	757·9	752·75	757·0	756·4	762·7	758·8
	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.
CO_2	4·69	4·86	4·70	4·86	5·12	5·14	4·89
O	16·01	15·51	15·69	15·57	15·43	15·44	15·61
N	79·30	79·63	79·61	79·57	79·45	79·42	79·50
O consumed	1696	1858	1749	1843	3305	1766	—
CO_2 produced	1547	1593	1487	1579	2954	1593	—
Relation	0·912	0·857	0·850	0·857	0·898	0·902	0·879
O absorbed	149	265	262	264	351	173	—
O absorbed per minute	21·3	34·9	36·7	36·7	27	24·8	30·2
O absorbed on 100 air expired	0·44	0·79	0·80	0·79	0·59	0·55	0·66
O absorbed on 100 O inspired	2·09	3·78	3·84	3·75	2·83	2·61	3·15
Weight O consumed per hour	20·84	20·58	21·09	22·02	21·81	21·81	21·37
Weight O per hour per kilo....	0·356	0·352	0·360	0·376	0·374	0·373	0·361
Weight CO_2 expired per minute	0·436	0·404	0·411	0·432	0·448	0·451	0·430

Experiments made Fasting.

	Exp 7.	Exp. 8.	Exp. 9.	Exp. 10.	Exp. 11.	Exp. 12.	Means fasting.	Total means.
Time after food...	4 hrs. after breakfast	4 hours after breakfast	4 hours after breakfast	4 hours 18 mins. after breakfast	4 hours 20 mins. after breakfast	4½ hours after breakfast	4 hours 11 mins. after food.	3 hours 13 mins. after food.
Laboratory temperature	14·6	14·6	16·1	16·1	15·2	23·3	16·7	15·9
Barometer	748·6	748·6	761·5	751·8	761·9	763·0	755·9	757·3
on 100 expired.		on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.
CO_2	4·71	4·72	4·53	4·71	4·98	5·14	4·80	4·84 CO_2
O	15·71	15·46	16·07	15·54	15·33	15·27	15·56	15·51 O
N.......................................	79·58	79·82	79·40	79·75	79·69	79·59	79·64	79·56 N
O consumed	1716	1818	1674	1821	1838	1774	—	—
CO_2 produced	1479	1484	1490	1494	1562	1533	—	—
Relation	0·862	0·816	0·890	0·820	0·850	0·864	0·850	0·864
O absorbed	237	334	184	327	276	242	—	—
O absorbed per minute	32·2	42·2	25·7	42·8	36·4	32·3	35·3	32·7
O absorbed on 100 air expired	0·74	1·03	0·54	1	0·86	0·79	0·83	0·74
O absorbed on 100 O inspired	3·52	4·94	2·60	4·79	4·12	3·77	3·96	3·55
Weight O consumed per hour	20·04	19·75	20·14	20·52	20·84	20·35	20·27	20·82
Weight O per hour per kilo....	0·342	0·338	0·344	0·351	0·356	0·348	0·346	0·355
Weight CO_2 expired per minute	0·396	0·369	0·411	0·386	0·406	0·403	0·395	0·412

Experiments on Ordinary Respiration showing the Composition of the Air expired and the Interchange of Gases.

Experiments made under the Influence of Food.

(Mr. Russell under experiment.)

Exp. 1. P. 157.		Exp. 2.	Exp. 3.	Exp. 4.	Exp. 5.	Exp. 6.	Means after food.
Time after food ...	2 hours after lunch.	2 hours 15 mins. after lunch.	2 hours 15 mins. after lunch.	2 hours after breakfast.	1 hour 44 mins. after lunch.	2 hours after lunch.	2 hours 2 mins.
Laboratory temperature	15·5	16·1	15·0	10·9	12·0	16·7	14·4
Barometer..........................	760·7	751·4	751·0	759·5	761·2	764·0	758·0
on 100 expired.		on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.
CO_2	5·52	5·67	5·71	5·69	5·90	5·51	5·67
O	14·56	14·93	14·71	14·95	14·42	14·94	14·75
N	79·92	79·40	79·58	79·36	79·68	79·55	79·58
O consumed	2205	2094	2106	4069	4442	2047	—
CO_2 produced	1804	1909	1855	3757	3851	1810	—
Relation..............................	0·818	0·911	0·881	0·923	0·867	0·884	0·881
O absorbed	401	185	251	312	591	237	—
O absorbed per minute	62·0	29·6	39·6	25·3	45·0	37·3	37·5
O absorbed on 100 expired ...	1·19	0·540	0·755	0·464	0·884	0·706	0·75
O absorbed on 100 O inspired	5·68	2·57	3·61	2·22	4·22	3·37	3·61
Weight O consumed per hour	29·33	28·82	28·6	28·42	29·06	27·73	28·66
Weight O per hour per kilo....	0·427	0·419	0·416	0·414	0·423	0·403	0·417
Weight CO_2 expired per minute	0·550	0·602	0·577	0·601	0·577	0·562	0·578

Experiments made Fasting.

Exp. 7. P. 39.		Exp. 8.	Exp. 9.	Exp. 10.	Exp. 11.	Exp. 12.	**Means fasting.**	**Total means.**
Time after food, 4 hours 35 mins. after breakfast.		4 hours 5 mins. after lunch.	4 hours 25 mins. after breakfast.	4 hours 15 mins after breakfast.	4 hours 35 mins. after breakfast.	4 hours 45 mins. after breakfast	**4 hours 23 mins. after food.**	**3 hours 12 mins. after food.**
Laboratory temperature	15·5	16·7	15·3	18·4	16·0	21·1	**16·4**	**15·4**
Barometer	764·6	764·0	765·5	749·5	748·2	765·5	**758·4**	**758·2**
on 100 expired.		on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	on 100 expired.	**on 100 expired.**	**on 100 expired.**
CO_2	5·52	5·38	5·50	5·8	5·78	5·84	**5·64**	**5·63 CO_2**
O	14·68	15·26	14·84	14·68	14·39	14·53	**14·73**	**14·76 O**
N	79·80	79·36	79·66	79·52	79·83	79·63	**79·63**	**79·60 N**
O consumed	2112	1887	1960	2066	2076	2062	—	—
CO_2 produced	1782	1733	1698	1858	1763	1816	—	—
Relation	0·844	0·918	0·866	0·899	0·849	0·881	**0·876**	**0·878**
O absorbed	330	154	262	208	313	246	—	—
O absorbed per minute	41·68	23·45	36·4	27·61	40·3	30·69	**33·35**	**25·42**
O absorbed on 100 expired ...	0·998	0·469	0·831	0·639	1·008	0·777	**0·79**	**0·77**
O absorbed on 100 O inspired	4·77	2·24	3·97	3·05	4·82	3·71	**3·76**	**3·69**
Weight O consumed per hour	22·95	24·72	23·41	23·59	22 99	22·12	**23·30**	**25·98**
Weight O per hour per kilo...	0·334	0·360	0·341	0·343	0·335	0·322	**0·330**	**0·373**
Weight CO_2 expired per minute	0·446	0·520	0·465	0·486	0·417	0·446	**0·468**	**0·523**

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June 11, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Sir John Conroy, Mr. Edwin Bailey Elliott, Mr. Percy C. Gilchrist, Dr. William Dobinson Halliburton, Mr. John Edward Marr, Mr. Ludwig Mond, Professor Silvanus Phillips Thompson, and Captain Thomas Henry Tizard were admitted into the Society.

The following Papers were read :—

I. "On Some Test Cases for the Maxwell-Boltzmann Doctrine regarding Distribution of Energy." By SIR WILLIAM THOMSON, D.C.L., P.R.S. Received June 11, 1891.

1. Maxwell, in his article ('Phil. Mag.,' 1860) "On the Collision of Elastic Spheres," enunciates a very remarkable theorem, of primary importance in the kinetic theory of gases, to the effect that, in an assemblage of large numbers of mutually colliding spheres of two or of several different magnitudes, the mean kinetic energy is the same for equal numbers of the spheres irrespectively of their masses and diameters; or, in other words, the time-averages of the squares of the velocities of individual spheres are inversely as their masses. The mathematical investigation given as a proof of this theorem in that first article on the subject is quite unsatisfactory; but the mere enunciation of it, even if without proof, was a very valuable contribution to science. In a subsequent paper ("Dynamical Theory of Gases," 'Phil. Trans.' for May, 1866) Maxwell finds in his equation (34) ('Collected Works,' p. 47), as a result of a thorough mathematical investigation, the same theorem extended to include collisions between Boscovich points with mutual forces according to any law of distance, provided only that not more than two points are in collision (that is to say, within the distances of their mutual influence) simultaneously. Tait confirms Maxwell's original theorem for colliding spheres of different magnitudes in an interesting and important examination of the subject in §§ 19, 20, 21 of his paper "On the Foundations of the Kinetic Theory of Gases" ('Trans. R.S.E.' for May, 1866).

2. Boltzmann, in his "Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten" ('Sitzb. K.

Akad. Wien,' October 8, 1868), enunciated a large extension of this theorem, and Maxwell a still wider generalisation in his paper "On Boltzmann's Theorem on the Average Distribution of Energy in a System of Material Points" ('Cambridge Phil. Soc. Trans.,' May 6, 1878, republished in vol. 2 of Maxwell's 'Scientific Papers,' pp. 713—741), to the following effect (p. 716):—

"In the ultimate state of the system, the average kinetic energy of two given portions of the system must be in the ratio of the number of degrees of freedom of those portions."

Much disbelief and doubt has been felt as to the complete truth, or the extent of cases for which there is truth, of this proposition.

3. For a test case, differing as little as possible from Maxwell's original case of solid elastic spheres, consider a hollow spherical shell and a solid sphere—globule we shall call it for brevity—within the shell. I must first digress to remark that what has hitherto by Maxwell and Clausius and others before and after them been called for brevity an "elastic sphere," is not an elastic solid, capable of rotation and of elastic deformation; and therefore capable of an infinite number of modes of steady vibration, into which, of finer and finer degrees of nodal sub-division and shorter and shorter periods, all translational energy would, if the Boltzmann-Maxwell generalised proposition were true, be ultimately transformed by collisions. The "smooth elastic spheres" are really Boscovich point-atoms, with their translational inertia, and with, for law of force, zero force at every distance between two points exceeding the sum of the radii of the two balls, and infinite repulsion at exactly this distance. We may use Boscovich similarly for the hollow shell with globule in its interior, and so do away with all question as to vibrations due to elasticity of material, whether of the shell or of the globule. Let us simply suppose the mutual action between the shell and the globule to be nothing except at an instant of collision, and then to be such that their relative component velocity along the radius through the point of contact is reversed by the collision, while the motion of their centre of inertia remains unchanged.

4. For brevity, we shall call the shell and interior globule of § 3, a double molecule, or sometimes, for more brevity, a doublet. The "smooth elastic sphere" of § 3 will be called simply an atom, or a single atom; and the radius or diameter or surface of the atom will mean the radius or diameter or surface of the corresponding sphere. (This explanation is necessary to avoid an ambiguity which might occur with reference to the common expression "sphere of action" of a Boscovich atom.)

5. Consider now a vast number of atoms and doublets, enclosed in a perfectly rigid fixed surface, having the property of reversing the normal component velocity of approach of any atom or shell or doublet

at the instant of contact of surfaces, while leaving unchanged the absolute velocity of the centre of inertia of the two. Let any velocity or velocities in any direction or directions be given to any one or more of the atoms or of the shells or globules constituting the doublets. According to the Boltzmann-Maxwell doctrine, the motion will become distributed through the system, so that ultimately the time-average kinetic energy of each atom, each shell, and each globule shall be equal; and therefore that of each doublet double that of each atom. This is certainly a very marvellous conclusion; but I see no reason to doubt it on that account. After all, it is not obviously more marvellous than the seemingly well proved conclusion, that in a mixed assemblage of colliding single atoms, some of which have a million million times the mass of others, the smaller masses will ultimately average a million times the velocity of the larger. But it is not included in Maxwell's proof for single atoms of different masses [(34) of his "Dynamical Theory of Gases" referred to above]; and the condition that the globules enclosed in the shells are prevented by the shells from collisions with one another violates Tait's condition [(C) of § 18 of "Foundations of K.T. Gases"], "that there is perfectly free access for collision between each pair of particles whether of the same or of different systems." An independent investigation of such a simple and definite case as that of the atoms and doublets defined in §§ 3—5 is desirable as a test, or would be interesting as an illustration were test not needed, for the exceedingly wide generalisation set forth in the Boltzmann-Maxwell doctrine.

6. Next, instead of only a single globule within the shell of § 4, let there be a vast number. To fix ideas let the mass of the shell be equal to a hundred times the sum of the masses of the globules, and let the number of the globules be a hundred million million. Let two such shells be connected by a push-and-pull massless spring. Let all be given at rest, with the spring stretched to any extent; and then left free. According to the Boltzmann-Maxwell doctrine, the motion produced initially by the spring will become distributed through the system, so that ultimately the sum of the kinetic energies of the globules within each shell will be a hundred million million times the average kinetic energy of the shell. The average velocity* of the shell will ultimately be a hundred-millionth of the average velocity of the globules. A corresponding proposition in the kinetic theory of gases is that, if two rigid shells each weighing 1 gram, and containing a centigram of monatomic gas, be attached to the two prongs of a massless perfectly elastic tuning fork, and set to vibrate, the gas will become heated in virtue of its viscous resistance

* The "average velocity of a particle," irrespectively of direction, is (in the kinetic theory of gases) a convenient expression for the square root of the time-average of the square of its velocity.

to the vibration excited in it by the vibration of the shell, until nearly all the initial energy of the tuning fork is thus spent.

7. Going back to the double molecules of § 5, suppose the internal globule to be so connected by massless springs with the shell that the globule is urged towards the centre of the shell with a force simply proportional to the distance between the centres of the two. This arrangement, which I gave in my Baltimore Lectures, in 1884, as an illustration for vibratory molecules embedded in ether, would be equivalent to two masses connected by a massless spring, if we had only motions in one line to consider; but it has the advantage of being perfectly isotropic, and giving for all motions parallel to any fixed line exactly the same result as if there were no motion perpendicular to it. When a pair of masses connected by a spring strikes a fixed obstacle or a movable body, with the line of their centres not exactly perpendicular to the tangent plane of contact, it is caused to rotate. No such complication affects our isotropic doublet. An assemblage of such doublets being given moving about within a rigid enclosing surface, will the ultimate statistics be, for each doublet,* equal average kinetic energies of motion of centre of inertia, and of relative motion of the two constituents?

* This implies equal average kinetic energies of the two constituents; and, conversely, equal average kinetic energies of the two constituents, except in the case of their masses being equal, implies the equality stated in the text. Let u, u' be absolute component velocities of two masses, m, m', perpendicular to a fixed plane; U the corresponding component velocity of their centre of inertia; and r that of their mutual relative motion. We have

$$u = \mathrm{U} - \frac{m'r}{m+m'}, \qquad u' = \mathrm{U} + \frac{mr}{m+m'}, \; \ldots\ldots\ldots\ldots \; (1);$$

whence
$$mu^2 - m'u'^2 = (m-m')\left[\mathrm{U}^2 - \frac{mm'r^2}{(m+m')^2}\right] - \frac{4mm'}{m+m'}\mathrm{U}r \; \ldots\ldots\ldots \; (2).$$

Now suppose the time-average of Ur to be zero. In every case in which this is so we have, by (2),

$$\text{Time-av.}\left\{mu^2 - m'u'^2\right\} = (m-m') \times \text{Time-av.}\left\{\mathrm{U}^2 - \frac{mm'r^2}{(m+m')^2}\right\} \ldots\; (3).$$

Hence in any case in which

$$\text{Time-av. } mu^2 = \text{Time-av. } m'u'^2 \; \ldots\ldots\ldots\ldots\ldots\ldots \; (4)$$

we have
$$(m-m') \times \text{Time-av.}\left\{\mathrm{U}^2 - \frac{mm'r^2}{(m+m')^2}\right\} = 0 \; \ldots\ldots\ldots\ldots \; (5),$$

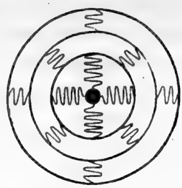
and therefore, except when $m = m'$, we must have

$$\text{Time-av. } (m+m')\,\mathrm{U}^2 = \text{Time-av. } \frac{mm'r^2}{m+m'} \; \ldots\ldots\ldots\ldots \; (6),$$

which proves the proposition, because, as we readily see from (1), $\frac{1}{2}\,mm'r^2/(m+m')$ is, in every case, the kinetic energy of the relative motions, $u-\mathrm{U}$, and $\mathrm{U}-u'$.

8. If we try to answer this question synthetically, we find a complex and troublesome problem in the details of all but the very simplest case of collision which can occur, which is direct collision between two not previously vibrating doublets, or any collision of one not previously vibrating doublet against a fixed plane. In this case, if the masses of globule and shell are equal, a complete collision consists of two impacts at an interval of time equal to half the period of free vibration of the doublet, and after the second impact there is separation without vibration, just as if we had had single spheres instead of the doublets. But in oblique collision between two not previously vibrating doublets, even if the masses of shell and globule are equal, we have a somewhat troublesome problem to find the interval between the two impacts, *when there are two*, and to find the final resulting vibration. When the component relative motion parallel to the tangent plane of the first impact exceeds a certain value depending on the radius of the outer surface of the shell, the period of free vibration of the doublets, and the relative velocity of approach; there is no second impact, and the doublets separate with no relative velocity perpendicular to the tangent plane, but each with the energy of that component of its previous motion converted into vibrational energy. When the mass of the shell is much smaller than the mass of the interior globule, almost every collision will consist of a large number of impacts. It seems exceedingly difficult to find how to calculate true statistics of these chattering collisions, and arrive at sound conclusions as to the ultimate distribution of energy in any of the very simplest cases other than Maxwell's original case of 1860; but, if the Boltzmann-Maxwell generalised doctrine is true, we ought to be able to see its truth as essential, with special clearness in the simplest cases, even without going through the full problem presented by the details. I can find nothing in Maxwell's latest article on the subject ('Camb. Phil. Trans.,' May 6, 1878), or in any of his previous papers, proving an affirmative answer to the question of § 7.

9. Going back to § 6, let the globules be initially distributed as nearly as may be homogeneously through the hollow; let each globule be connected with neighbours by massless springs; and let all the globules which are near the inner surface of the shell be connected with it also by massless springs. Or let any number of smaller shells be enclosed within our outer shell, and connected by massless springs as represented by the accompanying diagram, taken from a reprint of my Baltimore lectures now in progress. Let two such outer shells, given at rest with their systems of globules in equilibrium within them, be connected by massless springs, and be started in motion, as were the shells of § 6. There will not now be the great loss of energy from the vibration of the shells which there was in § 6. On the contrary, the ultimate average kinetic energy of the



whole two hundred million million globules will be certainly small in comparison with the ultimate average kinetic energy of the single shell. It may be because each globule of § 6 is free to wander that the energy is lost from the shell in that case, and distributed among them. There is nothing vague in their motion allowing them to take more and more energy, now when they are connected by the massless springs. If we suppose the motions infinitesimal, or if, whatever their ranges may be, all forces are in simple proportion to displacements, the elementary dynamical theorem of *fundamental modes* shows how to find determinately each of the 600 million million and six simple harmonic vibrations of which the motion resulting from the prescribed initial circumstances is constituted. It tells us that the sum of the potential and kinetic energies of each mode remains always of constant value, and that the time-average of the changing kinetic energy during its period is half of this constant value. Without fully solving the problem for the 600 million million and six co-ordinates, it is easy to see that the gravest fundamental mode of the motion actually produced in the prescribed circumstances differs but little in period and energy from the single simple harmonic vibration which the two shells would take if the globules were rigidly connected to them, or were removed from within them, and the other initial circumstances were those of § 6. But this conclusion depends on the forces being *rigorously* in simple proportion to displacements.

10.* In no real case could they be so, and if there is any deviation from the simple proportionality of force to displacement, the independent superposition of motions does not hold good. We have still a theorem of fundamental modes, although, so far as I know, this theory has not yet been investigated.† For any stable system moving with a given sum, E, of potential and kinetic energies, there must in general be *at least as many fundamental modes of rigorously periodic motion as there are freedoms* (or independent variables). But the configuration of each fundamental mode is now not *generally* similar

* Sections 10 to 17 added July 10, 1891.

† It is similar for *adynamic* cases, that is to say, cases in which there is no potential energy, as, for example, a particle constrained to remain on a surface and moving along a geodetic line under the influence of no "applied" force.

for different values of E; and superposition of different fundamental modes, whether with the same or with different values of E, *has now no meaning.* It seems to me probable that every fundamental mode is essentially unstable. It is so if Maxwell's fundamental assumption* "that the system if left to itself in its actual state of motion, will, sooner or later, pass through every phase which is consistent with the equation of energy" is true. It seems to me quite probable that this assumption *is* true, provided the "actual state of motion" is not exactly, as to position and velocity, a configuration of some one of the fundamental modes of rigorously periodic motion, and provided also that the "system" has not any exceptional character, such as those indicated by Maxwell for cases in which he warns† us that his assumption does not hold good.

11. But, conceding Maxwell's fundamental assumption, I do not see in the mathematical workings of his paper‡ any proof of his conclusion "that the average kinetic energy corresponding to any one of the variables is the same for every one of the variables of the system." Indeed, as a general proposition its meaning is not explained, and seems to me inexplicable. The reduction of the kinetic energy to a sum of squares§ leaves the several parts of the whole with no correspondence to any defined or definable set of independent variables. What, for example, can the meaning of the conclusion|| be for the case of a jointed pendulum? (a system of two rigid bodies, one supported on a fixed, horizontal axis and the other on a parallel axis fixed relatively to the first body, and both acted on only by gravity). The conclusion is quite intelligible, however (but is it true?), when the kinetic energy is expressible as a sum of squares of rates of change of single co-ordinates each multiplied by a function of all, or of some, of the co-ordinates.¶ Consider, for example, the still easier case of these coefficients constant.

12. Consider more particularly the easiest case of all, motion of a single particle in a plane, that is the case of just two independent variables, say, x, y; and kinetic energy equal to $\frac{1}{2}(\dot{x}^2+\dot{y}^2)$. The equations of motion are

$$\frac{d^2x}{dt^2}=-\frac{dV}{dx}, \qquad \frac{d^2y}{dt^2}=-\frac{dV}{dy},$$

* "Scientific Papers," Vol. II, p. 714.

† *Ibid.*, pp. 714, 715.

‡ *Ibid.*, pp. 716—726.

§ *Ibid.*, p. 722.

|| Or of Maxwell's "$\dot{b}$," in p. 723.

¶ [It may be untrue for one set of co-ordinates, though true for others. Consider, for example, uniform motion in a circle. For all systems of rectilineal rectangular co-ordinates (x, y), time-av. $\dot{x}^2$ = time-av. $\dot{y}^2$; but for polar co-ordinates (r, θ) we have *not* time-av. $\dot{r}^2$ equal to time-av. $r^2\dot{\theta}^2$.—W. T., July 21, 1891.]

where V is the potential energy, which may be any function of x, y, subject only to the condition (required for stability) that it is essentially positive (its least value being, for brevity, taken as zero). It is easily proved that, with any given value, E, for the sum of kinetic and potential energies there are two determinate modes of periodic motion; that is to say, there are two finite closed curves such that if m be projected from any point of either with velocity equal to $\surd[2(\mathrm{E}-\mathrm{V})]$ in the direction, eitherwards, of the tangent to the curve, its path will be exactly that curve. In a very special class of cases there are only two such periodic motions, but it is obvious that there are more than two in other cases.

13. Take, for example,

$$\mathrm{V} = \tfrac{1}{2}(\alpha^2x^2+\beta^2y^2+cx^2y^2).$$

For all values of E we have

$$\left.\begin{array}{l} x = a\cos(\alpha t-e) \\ y = 0 \end{array}\right\} \text{ and } \left.\begin{array}{l} y = 0 \\ x = b\cos(\beta t-f) \end{array}\right\}$$

as two fundamental modes. When E is infinitely small we have only these two; but for any finite value of E we have clearly an infinite number of fundamental modes, and *every mode* differs infinitely little from being a fundamental mode. To see this let m be projected from any point N in OX, in a direction perpendicular to OX, with a velocity equal to $\surd(2\mathrm{E}-\alpha^2\mathrm{ON}^2)$. After a sufficiently great number of crossings and re-crossings across the line X′OX, the particle will cross this line very nearly at right angles, at some point, N′. Vary the position of N very slightly in one direction or other, and re-project m from it perpendicularly and with proper velocity; till (by proper "trial and error" method) a path is found, which, after still the same number of crossings and re-crossings, crosses exactly at right angles at a point N″, very near the point N′. Let m continue its journey along this path and, after just as many more crossings and re-crossings, it will return *exactly* to N, and cross OX there, *exactly* at right angles. Thus the path from N to N″ is exactly half an orbit, and from N″ to N the remaining half.

14. When $c\mathrm{E}/(\alpha^2\beta^2)$ is a small numeric, the part of the kinetic energy expressed by $\frac{1}{2}cx^2y^2$ is very small in comparison with the total energy, E. Hence the path is at every time very nearly the resultant of the two primary fundamental modes formulated in § 13; and an interesting problem is presented, to find (by the method of the "variation of parameters") a, e, b, f, slowly varying functions of t, such that

$$x = a\ \sin(\alpha t-e), \qquad y = b\ \sin(\beta t-f),$$

$$\dot{x} = a\alpha\cos(\alpha t-e), \qquad \dot{y} = b\beta\cos(\beta t-f),$$

shall be the rigorous solution, or a practical approximation to it. Careful consideration of possibilities in respect to this case [$cE/(\alpha^2\beta^2)$ very small] seem thoroughly to confirm Maxwell's fundamental assumption quoted in § 11; and that it is correct whether $cE/(\alpha^2\beta^2)$ be small or large seems exceedingly probable, or quite certain.

15. But it seems also probable that Maxwell's *conclusion*, which for the case of a material point moving in a plane is

$$\text{Time-av. } \dot{x}^2 = \text{Time-av. } \dot{y}^2 \quad \ldots\ldots\ldots\ldots\ldots \quad (1)$$

is not true when α^2 differs from β^2. It is certainly not proved. No dynamical principle except the equation of energy,

$$\tfrac{1}{2}(\dot{x}^2+\dot{y}^2) = E-V \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2),$$

is brought into the mathematical work of pp. 722—725, which is given by Maxwell as proof for it. Hence any arbitrarily drawn curve might be assumed for the path without violating the dynamics which enters into Maxwell's investigation; and we may draw curves for the path such as to satisfy (1), and curves not satisfying (1), but all traversing the whole space within the bounding curve

$$\tfrac{1}{2}(\alpha^2x^2+\beta^2y^2+cx^2y^2) = E \quad \ldots\ldots\ldots\ldots\ldots \quad (3),$$

and all satisfying Maxwell's fundamental assumption (§ 11).

16. The meaning of the question is illustrated by reducing it to a purely geometrical question regarding the path, thus:—calling θ the inclination to x of the tangent to the path at any point xy, and q the velocity in the path, we have

$$\dot{x} = q\cos\theta, \qquad \dot{y} = q\sin\theta \quad \ldots\ldots\ldots\ldots \quad (4),$$

and therefore, by (2) $\quad q = \sqrt{\{2(E-V)\}} \quad \ldots\ldots\ldots\ldots\ldots \quad (5).$

Hence, if we call s the total length of curve travelled,

$$\int \dot{x}^2 dt = \int q\cos^2\theta\, q\,dt = \int \sqrt{\{2(E-V)\}}\cos^2\theta\, ds \ldots\ldots \quad (6);$$

and the question of § 15 becomes, Is or is not

$$\frac{1}{S}\int_0^S ds\sqrt{\{2(E-V)\}}\cos^2\theta = \frac{1}{S}\int_0^S ds\sqrt{\{2(E-V)\}}\sin^2\theta \,? \quad \ldots \quad (7),$$

where S denotes so great a length of path that it has passed a great number of times very near to every point within the boundary (3), very nearly in every direction.

17. Consider now separately the parts of the two members of (7) derived from portions of the path which cross an infinitesimal area $d\sigma$ having its centre at (x, y). They are respectively

$$\sqrt{\{2(E-V)\}}\,d\sigma\int_0^{\pi} N d\theta \cos^2\theta, \quad \text{and} \quad \sqrt{\{2(E-V)\}}\,d\sigma\int_0^{\pi} N d\theta \sin^2\theta \quad \ldots\ldots \ (8),$$

where $N d\theta$ denotes the number of portions of the path, per unit distance in the direction inclined $\frac{1}{2}\pi+\theta$ to x, which pass eitherwards across the area in directions inclined to x at angles between the values $\theta-\frac{1}{2}d\theta$ and $\theta+\frac{1}{2}d\theta$. The most general possible expression for N is, according to Fourier,

$$\left.\begin{aligned} N = A_0 + A_1\cos 2\theta + A_2\cos 4\theta + \&c.\\ + B_1\sin 2\theta + B_2\sin 4\theta + \&c.\end{aligned}\right\} \ldots\ldots\ldots \ (9).$$

Hence the two members of (8) become respectively

$$\sqrt{\{2(E-V)\}}\,d\sigma\,\tfrac{1}{2}\pi(A_0+\tfrac{1}{2}A_1), \quad \text{and} \quad \sqrt{\{2(E-V)\}}\,d\sigma\,\tfrac{1}{2}\pi(A_0-\tfrac{1}{2}A_1) \quad \ldots\ldots\ldots \ (10).$$

Remarking that A_0 and A_1 are functions of x, y, and taking $d\sigma = dx dy$, we find, from (10), for the two totals of (7) respectively

$$\left.\begin{aligned} &\tfrac{1}{2}\pi\iint dx\,dy(A_0+\tfrac{1}{2}A_1)\sqrt{[2(E-V)]}\\ \text{and} \quad &\tfrac{1}{2}\pi\iint dx\,dy(A_0-\tfrac{1}{2}A_1)\sqrt{[2(E-V)]}\end{aligned}\right\} \ldots\ldots\ldots \ (11),$$

where $\iint dx dy$ denotes integration over the whole space enclosed by (3). These quantities are equal if and only if $\iint dx\,dy\,A_1$ vanishes; it does so, clearly, if $\alpha = \beta$; but it seems improbable that, except when $\alpha = \beta$, it can vanish generally; and unless it does so, our present test case would disprove the Boltzmann-Maxwell general doctrine.

II. "On Electrical Evaporation." By WILLIAM CROOKES, F.R.S. Received June 4, 1891.

It is well known that when a vacuum tube is furnished with internal platinum electrodes, the adjacent glass, especially near the negative pole, speedily becomes blackened, owing to the deposition of metallic platinum. The passage of the induction current greatly stimulates the motion of the residual gaseous molecules; those condensed upon and in the immediate neighbourhood of the negative pole are shot away at an immense speed in almost straight lines, the speed varying with the degree of exhaustion and with the intensity of the induced current. Platinum being used for the negative pole,

not only are the gaseous molecules shot away from the electrode, but the passage of the current so affects the normal molecular motions of the metal as to remove some of the molecules from the sphere of attraction of the mass, causing them to fly off with the stream of gaseous molecules proceeding from the negative pole, and to adhere to any object near it. This property was, I believe, first pointed out by Dr. Wright, of Yale College, and some interesting experiments are described by him in 'The American Journal of Science and Arts.'* The process has been much used for the production of small mirrors for physical apparatus.

This electrical volatilisation or evaporation is very similar to ordinary evaporation by the agency of heat. Cohesion in solids varies according to physical and chemical constitution; thus every kind of solid matter requires to be raised to a certain temperature before the molecules lose their fixity of position and are rendered liquid, a result which is reached at widely different temperatures. If we consider a liquid at atmospheric pressure,—say, for instance, a basin of water in an open room,—at molecular distances the boundary surface between the liquid and the superincumbent gas will not be a plane, but turbulent like a stormy ocean. The molecules at the surface of the liquid dart to and fro, rebound from their neighbours, and fly off in every direction. Their initial velocity may be either accelerated or retarded according to the direction of impact. The result of a collision may drive a molecule in such a direction that it remains part and parcel of the liquid; on the other hand, it may be sent upwards without any diminution of speed, and it will then be carried beyond the range of attraction of neighbouring molecules and fly off into and mingle with the superincumbent gas. If a molecule of the liquid has been driven at an angle with a velocity not sufficient to carry it beyond the range of the molecular attraction of the liquid it may still escape, since, in its excursion upwards, a gaseous molecule may strike it in the right direction, and its temporary visit may be converted into permanent residence.

The intrinsic velocity of the molecules is intensified by heat and diminished by cold. If, therefore, we raise the temperature of the water without materially increasing that of the surrounding air, the excursions of the molecules of the liquid are rendered longer and the force of impact greater, and thus the escape of molecules into the upper region of gas is increased, and we say that evaporation is augmented.

If the initial velocities of the liquid molecules can be increased by any other means than by raising the temperature, so that their escape into the gas is rendered more rapid, the result may be called "evaporation" just as well as if heat had been applied.

* Third Series, vol. 12, p. 49, January, 1877, and vol. 14, p. 169, September, 1877.

Hitherto I have spoken of a liquid evaporating into a gas; but the same reasoning applies equally to a solid body. But whilst a solid body like platinum requires an intense heat to enable its upper stratum of molecules to pass beyond the sphere of attraction of the neighbouring molecules, experiment shows that a very moderate amount of negative electrification superadds sufficient energy to enable the upper stratum of metallic molecules to fly beyond the attractive power of the rest of the metal.

If a gaseous medium exists above the liquid or solid, it prevents to some degree the molecules from flying off. Thus both ordinary and electrical evaporation are more rapid in a vacuum than at the ordinary atmospheric pressure.

I have recently made some experiments upon the evaporation of different substances under the electric stress.

Evaporation of Water.—A delicate balance was taken, and two very shallow porcelain dishes were filled with acidulated water and balanced on the pans. Dipping into each dish—touching the liquid, but not the dish—was a platinum wire, one connected with the induction coil and the other insulated. The balance was left free to move, but was not swinging, the pointer resting at the centre of the scale. The water in connection with the coil was first made positive. After $1\frac{3}{4}$ hours there was scarcely any difference between the weight of the insulated water and that which had been exposed to the positive current. Equilibrium being restored, the current was reversed, the negative current being kept on the dish for two hours. At the end of this time the electrified water was decidedly lighter. After having again restored equilibrium, the electrification of the dishes was reversed, *i.e.*, the one that had before been insulated was made negative and the other one was insulated. In an hour the electrified water had become decidedly lighter than the insulated water. The experiment was performed in a room of uniform temperature, and any draught was prevented by the glass case of the balance. In a subsequent experiment in which the quantities were weighed, it was found that negatively electrified water lost in $1\frac{1}{2}$ hours 1/1000th part of its weight more than did insulated water.

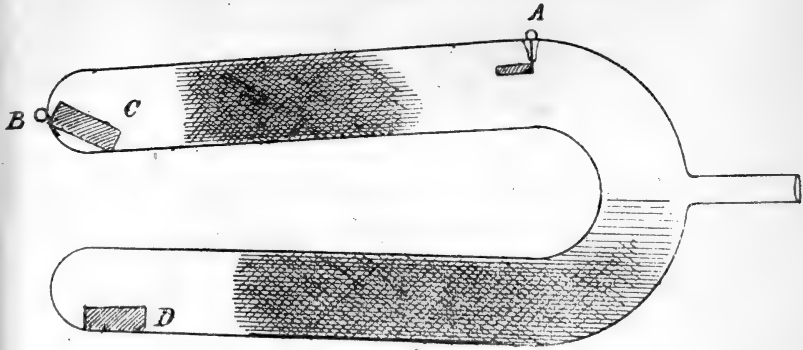
This experiment shows that the disturbing influence which assists evaporation is peculiar to the negative pole even at atmospheric pressures.

The metal cadmium was next experimented upon.

Evaporation of Cadmium.—If the flying-off of the metal of the negative pole is similar to evaporation or volatilisation, the operation should be accelerated by heat.

A tube was made as shown in fig. 1. A and B are the platinum poles sealed through the glass. C and D are two blocks of metallic cadmium of the same size and weight. The piece C is in contact

Fig. 1.



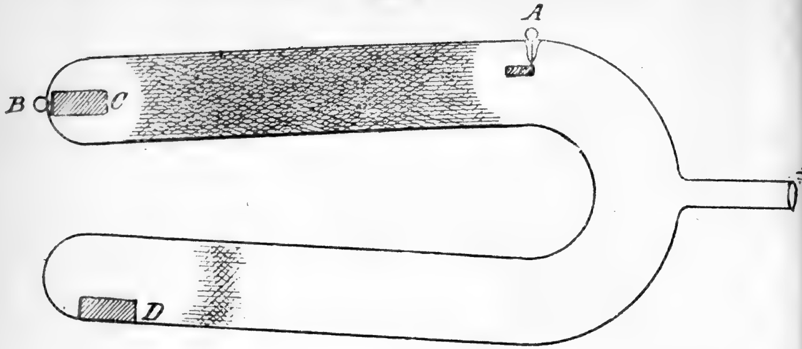
with the pole B, which in the experiment was always kept negative, the pole A being positive. When the exhaustion was such that the passage of the current gave green phosphorescence over the glass, heat was applied simultaneously to both ends of the U-shaped tube by means of a gas-burner and air-bath, so that one piece of cadmium was at the same temperature as the other. The current was then applied and was kept on for about an hour, and it was remarkable that no metal was deposited in the neighbourhood of the positive pole, the surrounding portion of the tube being quite clean, while the corresponding part of the other limb of the tube, having no electrodes, was thickly coated, the appearance being shown in the drawing.

As the temperature was high, metal had distilled off from both lumps; hence there was no visible difference in the amount of the deposit in the two sides. It is evident that, to render the electrical action most visible, the temperature should be kept short of the normal volatilising point.

In the next experiment an exactly similar tube was used; the vacuum was such that the green phosphorescence of the glass was well seen, the temperature was kept just below the melting point of cadmium, and the current was kept on for an hour. On examining the tube at the end of this time, the appearance was as seen in fig. 2. A considerable deposit had taken place on the end of the tube near the negative pole, the space round the positive pole was clear, while in the limb of the tube where no electricity had been passing only a very little deposit of metal was seen, as shown in the figure.

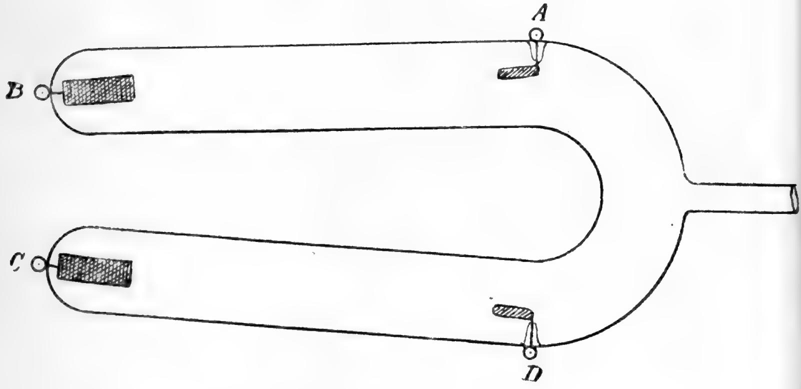
The temperature in this experiment having been kept below the melting point, had no electricity been applied, there would have been very little, if any, evaporation. The amplitude of the molecular oscillations was increased by the rise of temperature, but not suffi-

FIG. 2.



ciently to allow many of the molecules to pass beyond the sphere of attraction of the mass. When, however, the current was turned on, the oscillations were increased sufficiently to carry some of the molecules beyond their spheres of attraction and hence into the vacuous space above. As in the water experiment, this only happens at the negative pole. It would seem that, even after having been *removed* from the rest of the mass, the on-rushing stream of gaseous molecules is necessary to carry the metallic molecules away, and, as I shall presently show, even then they very quickly drop out of the ranks and deposit on the walls of the tube.

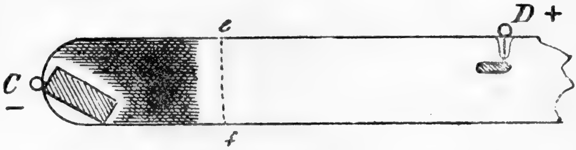
FIG. 3.



Another tube was made as shown in fig. 3. The poles A, B, C, D were platinum wires sealed through the glass, A and D having aluminium poles covering the platinum wire. In the ends of the

tube, and touching the poles B and C, were two pieces of cadmium of the same size and shape. The tube was exhausted to the phosphorescent point, and the current was turned on, C being made negative and D positive. No heat was applied. The current was kept on for about half an hour, until a good deposit of metal had been deposited on the glass, the appearance being as shown in fig. 4,

Fig. 4.



the glass near the pole C being coated with metal, while the glass round the pole D was clean. The outer boundary of the dark space during the experiment is shown by the dotted line *ef*.

The pole B was now made positive and the pole A negative, the current being kept on for another half hour. At the end of the time the only additional effect was a slight darkening round the lump of cadmium, in the same place as, but very much fainter than, the deposit shown in fig. 5. This is probably due to a little leakage of negative

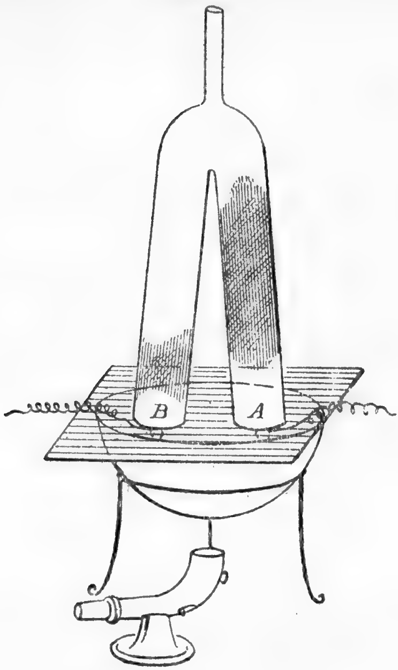
Fig. 5.

discharge from the positive pole. The experiment shows that positive electrification does not cause the metal sensibly to volatilise.

In these experiments no estimation was made of the weight of metal removed, and the cadmium only rested by its own weight upon the platinum wires that had been sealed through the glass. To render the experiment quantitative, and at the same time to remove any disturbing effect that might be caused by heating at the point of indifferent contact, the following experiments were made:—

A U-shaped tube, shown in fig. 6, had a platinum pole sealed in each end. 6 grains of pure cadmium were put into each limb and fused round the platinum wire. The ends of the tube were then put into an air-bath, and kept at a temperature of 200° C. during the

Fig. 6.



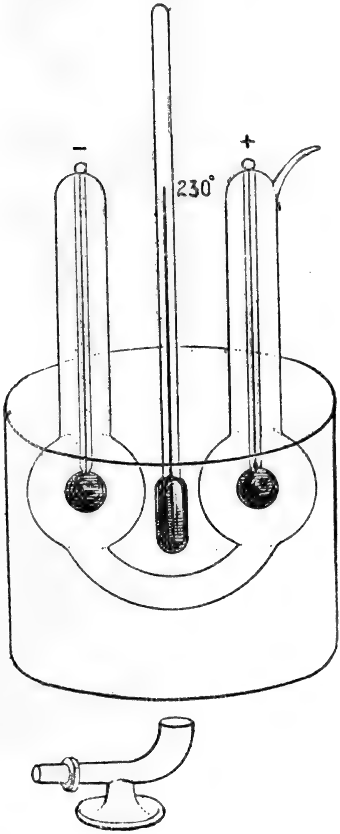
continuance of the experiment.* The exhaustion remained at 0·00076 mm., or 1 M. The induction current was kept going for thirty-five minutes, the pole A being negative and B positive. At the end of this time it was seen that most of the cadmium had disappeared from the negative pole, leaving the platinum wire clean, no metal being deposited near it, and the molecules appearing to have been shot off to a distance of about $\frac{3}{4}$ inch. The appearance of the positive pole was very different; scarcely any of the cadmium had been volatilised, and the condensed metal came almost close to the pole. The tube was opened, and the remaining wires and metal were weighed. The cadmium was then dissolved off the poles in dilute acid; the residue was washed, dried, and weighed.

	Positive pole.	Negative pole.
Original weight of cadmium	6·00 grs.	6·00 grs.
Cadmium remaining on the pole ...	3·65 ,,	0·25 ,,
Cadmium volatilised in 35 mins. ..	2·35 ,,	5·75 ,,

* Cadmium melts at 320° and boils at 860°.

The difference between the amount of cadmium driven from the two poles having proved to be so decided, another experiment was tried in a tube so arranged that the metal could be more easily weighed before and after the experiment. The apparatus is shown in fig. 7. A tube was blown U-shaped, having a bulb in each limb.

FIG 7.



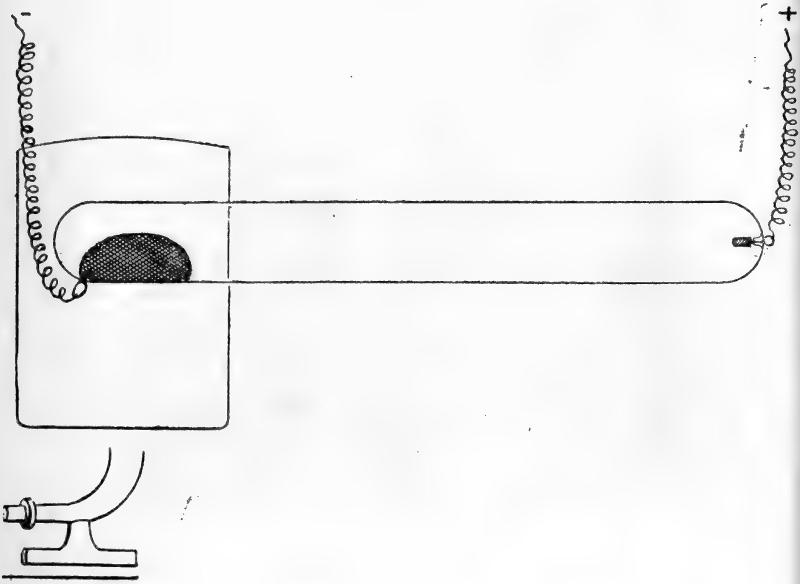
The platinum poles were, as before, at the extremities of each limb, and in each bulb was suspended from a small platinum hook a small lump of cadmium, the metal having been cast on to the wire. The wires were each weighed with and without the cadmium. The tube was exhausted, and the lower half of the tube was enclosed in a metal pot containing paraffin wax, the temperature being kept at 230° C.

during the continuance of the experiment. A deposit around the negative pole took place almost immediately, and in five minutes the bulb surrounding it was opaque with deposited metal. The positive pole with its surrounding luminosity could be easily seen the whole time. In thirty minutes the experiment was stopped, and after all was cold the tube was opened and the wires weighed again. The results were as follows:—

	Positive pole.	Negative pole.
Original weight of cadmium	9·34 grs.	9·38 grs.
Weight after experiment	9·25 ,,	1·86 ,,
Cadmium volatilised in 30 mins. ..	0·09 ,,	7·52 ,,

Finding that cadmium volatilised so readily under the action of the induction current, a large quantity, about 350 grs., of the pure metal, was sealed up in a tube arranged as in fig. 8, and the end of

Fig. 8.



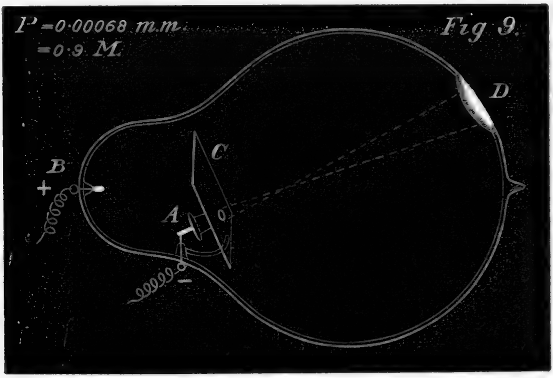
the tube containing the metal was heated to a little above the melting point; the molten metal being made the negative pole, in a few hours the whole quantity had volatilised and condensed in a thick layer on the far end of the tube, near, but not touching, the positive pole.

Volatilisation of Silver.—Silver was the next metal experimented

upon. The apparatus was similar to that used for the cadmium experiments (fig. 7). Small lumps of pure silver were cast on the ends of platinum wires, and suspended to the inner ends of platinum terminals passing through the glass bulb. The platinum wires were protected by glass, so that only the silver balls were exposed. The whole apparatus was enclosed in a metal box lined with mica, and the temperature was kept as high as the glass would allow without softening. The apparatus was exhausted to a dark space of 3 mm., and the current was kept on for $1\frac{1}{2}$ hours. The weights of silver, before and after the experiment, were as follows:—

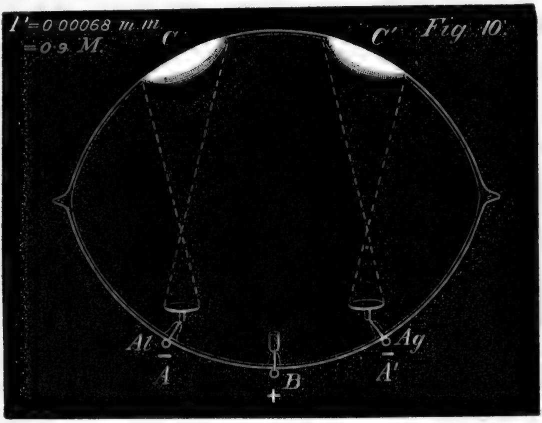
	Positive pole.	Negative pole.
Original weight of silver........	18·14 grs.	24·63 grs.
Weight after the experiment....	18·13 ,,	24·44 ,,
Silver volatilised in $1\frac{1}{2}$ hours....	0·01 ,,	0·19 ,,

It having been found that silver volatilised readily from the negative pole in a good vacuum, experiments were instituted to ascertain whether the molecules of metal shot off from the pole were instrumental in producing phosphorescence. A glass apparatus was made as shown in fig. 9. A pear-shaped bulb of German glass



has, near the small end, an inner concave negative pole, A, of pure silver, so mounted that its inverted image is thrown upon the opposite end of the tube. In front of the pole is a screen of mica, having a small hole in the centre, so that only a narrow pencil of rays from the silver pole can pass through, forming a bright spot of phosphorescence, D, at the far end of the bulb. The exhaustion was pushed to a high point, 0·00068 mm., or 0·9 M. The current from an induction coil was allowed to pass continuously for some hours, the silver

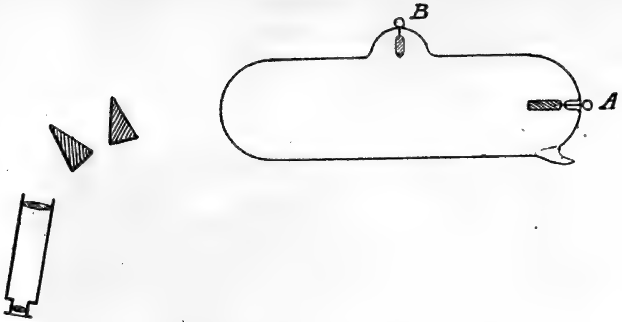
pole being kept negative, so as to drive off a certain portion of the silver electrode. On subsequent examination it was found that the silver had all been deposited in the immediate neighbourhood of the pole, whilst at the far end of the tube the spot D, that had been continuously glowing with phosphorescent light, was practically free from silver.



A tube was next made as shown in fig. 10. It had two negative poles connected together, A, A′, so placed as to project two luminous spots on the phosphorescent glass of the tube. One of the electrodes, A′, was of silver, a volatile metal; the other, A, was of aluminium, practically non-volatile. On connecting the two negative poles, A, A′, with one terminal of the coil, and the positive pole, B, with the other terminal, it was seen in the course of half an hour that a considerable quantity of metal had been projected from the silver negative pole, blackening the tube in its neighbourhood, while no projection of metallic particles took place from the aluminium positive pole. During the whole time of the experiment, however, the two patches of phosphorescent light, C and C′, had been glowing with exactly the same intensity, showing that the active agent in effecting phosphorescence was not the molecules of the solid projected from the poles, but the residual gaseous particles, or "radiant matter."

In the tubes hitherto made containing silver, it had not been easy to observe the spectrum of the negative pole, owing to the rapid manner in which the deposit obscured the glass. A special tube (fig. 11) was therefore devised, of the following character. The silver pole, A, was attached to the platinum pole at one end of the tube, and the aluminium positive pole, B, was at the side. The end of the tube opposite the silver pole was rounded, and the spectroscope was

FIG. 11.



arranged to observe the light of the volatilising silver "end on," as shown in the figure. In this way the deposit of silver offered no obstruction to the light, as none was deposited except on the sides of the tube surrounding the silver. At a vacuum giving a dark space of about 3 mm. from the silver, a greenish-white glow was seen to surround the metal. This glow gave a very brilliant spectrum. The spark from silver poles in air was brought into the same field of view as the vacuum glow, by means of a right-angled prism attached to the spectroscope, and the two spectra were compared. The two strong green lines of silver were visible in each spectrum; the measurements taken of their wave-lengths were 3344 and 3675, numbers which are so close to Thalén's numbers as to leave no doubt that they are the silver lines. At a pressure giving a dark space of 2 mm. the spectrum was very bright, and consisted chiefly of the two green lines and the red and green hydrogen lines. The intercalation of a Leyden jar into the circuit does not materially increase the brilliancy of the lines, but it brings out the well-known air lines. At this pressure not much silver flies off from the pole. At a higher vacuum, the luminosity round the silver pole gets less and the green lines vanish. At an exhaustion of about one-millionth of an atmosphere the luminosity is feeble, the silver pole has exactly the appearance of being red hot, and the volatilisation of the metal proceeds rapidly.*

* Like the action producing volatilisation, the "red heat" is confined to the superficial layers of molecules only. The metal instantly assumes, or loses, the appearance of red heat the moment the current is turned on or off, showing that, if the appearance is really due to a rise of temperature, it does not penetrate much below the surface. The extra activity of the metallic molecules necessary to volatilise them is, in these experiments, confined to the surface only, or the whole mass would evaporate at once, as when a metallic wire is deflagrated by the discharge of a powerful Leyden jar. When this extra activity is produced by artificial heat one of the effects is the emission of red light; so it is not unreasonable

If, for the negative electrode, instead of a pure metal such as cadmium or silver, an alloy was used, the different components might be shot off to different distances, and in this way make an electrical separation—a sort of fractional distillation. A negative terminal was formed of clean brass, and submitted to the electrical discharge *in vacuo;* the deposit obtained was of the colour of brass throughout, and on treating the deposit chemically I could detect no separation of its component metals, copper and zinc.

[A remarkable alloy of gold and aluminium, of a rich purple colour, has been kindly sent me by Professor Roberts-Austen. Gold being very volatile in the vacuum tube, and aluminium almost fixed, this alloy was likely to give different results from those yielded by brass, where both constituents fly off with almost equal readiness. The AuAl alloy had been cast in a clay tube in the form of a rod 2 cm. long and about 2 mm. in diameter. It was sealed in a vacuum tube as the negative pole, an aluminium pole being at the other side. Part of the alloy, where it joined the platinum wire passing through the glass, was closely surrounded with a narrow glass tube; a clean glass plate was supported about 3 mm. from the rod of alloy. After good exhaustion the induction current was passed, the alloy being kept negative. Volatilisation was very slight, but at the end of half an hour a faint purple deposit was seen both on the glass plate and on the walls of the tube. On removing the rod from the apparatus, it was seen that the portion which had been covered by the small glass tube retained its original purple appearance, while the part that had been exposed to electrical action had changed to the dull white colour of aluminium. Examined under the microscope, the whitened surface of the Austen alloy was seen to be pitted irregularly, with no trace of crystalline appearance. This experiment shows that from an alloy of gold and aluminium the gold is the first to volatilise under electrical influence, the aluminium being left behind. The purple colour of the deposit on glass is probably due to finely divided metallic gold. The first deposit from a negative pole of pure gold is pink; this changes to purple as the thickness increases. The purple then turns to green, which gets darker and darker until the metallic lustre of polished gold appears.—June 10.]

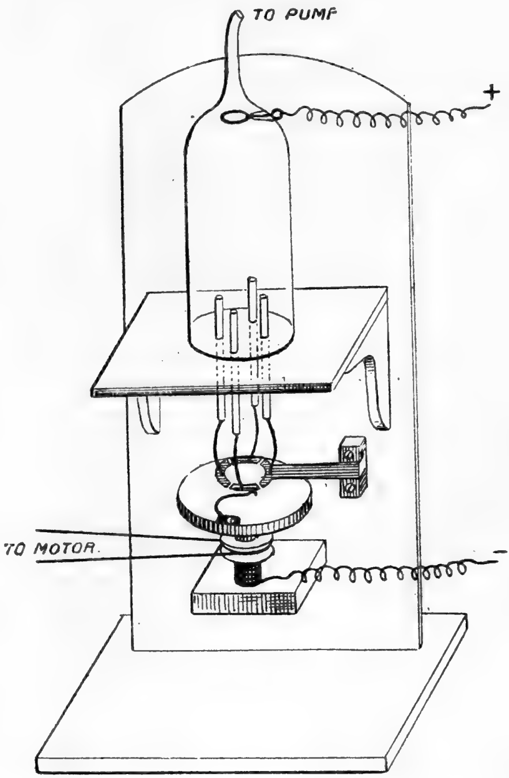
Returning to the analogy of liquid evaporation, if we take several liquids of different boiling points, put them under the same pressure, and apply the same amount of heat to each, the quantity passing

to imagine that when the extra activity is produced by electricity the emission of red light should also accompany the separation of molecules from the mass. In comparison with electricity heat is a wasteful agent for promoting volatilisation, as the whole mass must be raised to the requisite temperature to produce a surface action merely; whereas the action of electrification does not appear to penetrate much below the surface.

from the liquid to the gaseous state will differ widely in each case.

It was interesting to try a parallel experiment with metals, to find their comparative volatility under the same conditions of temperature, pressure, and electrical influence. It was necessary to fix upon one metal as a standard of comparison, and for this purpose I selected gold, its electrical volatility being great, and it being easy to prepare in a pure state.

Fig. 12.



An apparatus was made as in fig. 12. It is practically a vacuum tube with four negative poles at one end and one positive pole at the other. By a revolving commutator I was able to make electrical connection with each of the four negative poles in succession for exactly the same length of time (about six seconds); by this means

the variations in the strength of the current, the experiment lasting some hours, affected each metal alike.

The exposed surface of the various metals used as negative poles was kept uniform by taking them in the form of wires that had all been drawn through the same standard hole in the drawplate, and cutting them by gauge to a uniform length; the actual size used was 0·8 mm. in diameter, and 20 mm. long.

The comparison metal gold had to be used in each experiment; the apparatus thus enabled me to compare three different metals each time. The length of time that the current was kept on the revolving commutator in each experiment was eight hours, making two hours of electrification for each of the four negative electrodes; the pressure was such as to give a dark space of 6 mm.

The fusible metals, tin, cadmium, and lead, when put into the apparatus in the form of wires, very quickly melted. To avoid this difficulty a special form of pole was devised. Some small circular porcelain basins were made, 9 mm. diameter; through a small hole in the bottom a short length of iron wire, 0·8 mm. in diameter, was passed, projecting downwards about 5 mm.; the basin was then filled to the brim with the metal to be tested, and was fitted into the apparatus exactly in the same way as the wires; the internal diameter of the basins at the brim was 7 mm., and the negative metal filed flat was thus formed of a circular disc 7 mm. diameter. The standard gold pole being treated in the same way, the numbers obtained for the fusible metals can be compared with gold, and take their place in the table.

The following table of the comparative volatilities was in this way obtained, taking gold as = 100:—

Palladium	108·00
Gold	100·00
Silver	82·68
Lead	75·04
Tin	56·96
Brass....................	51·58
Platinum	44·00
Copper	40·24
Cadmium	31·99
Nickel	10·99
Iridium..................	10·49
Iron	5·50

In this experiment equal surfaces of each metal were exposed to the current. By dividing the numbers so obtained by the specific gravity of the metal, the following order is found:—

Palladium	9·00
Silver	7·88
Tin	7·76
Lead	6·61
Gold	5·18
Cadmium	3·72
Copper	2·52
Platinum	2·02
Nickel	1·29
Iron	0·71
Iridium	0·47

Aluminium and magnesium appear to be practically non-volatile under these circumstances.

The order of metals in the table shows at once that the electrical volatility in the solid state does not correspond with the order of melting points, of atomic weights, or of any other well-known constant. The experiment with some of the typical metals was repeated, and the numbers obtained did not vary materially from those given above, showing that the order is not likely to be far wrong.

It is seen in the above table that the electrical volatility of silver is high, while that of cadmium is low. In the two earlier experiments, where cadmium and silver were taken, the cadmium negative electrode in 30 minutes lost 7·52 grs., whilst the silver negative electrode in $1\frac{1}{2}$ hours only lost 0·19 gr. This apparent discrepancy is easily explained by the fact (already noted in the case of cadmium) that the maximum evaporation effect, due to electrical disturbance, takes place when the metal is at or near the point of liquefaction. If it were possible to form a negative pole *in vacuo* of molten silver, then the quantity volatilised in a given time would be probably much more than that of cadmium.

Gold having proved to be readily volatile under the electric current, an experiment was tried with a view to producing a larger quantity of the volatilised metal. A tube was made having at one end a negative pole composed of a weighed brush of fine wires of pure gold, and an aluminium pole at the other end.

The tube was exhausted and the current from the induction coil put on, making the gold brush negative; the resistance of the tube was found to increase considerably as the walls became coated with metal, so much so that, to enable the current to pass through, air had to be let in after a while, depressing the gauge $\frac{1}{2}$ mm.

The weight of the brush before experiment was 35·4940 grs. The induction current was kept on the tube for $14\frac{1}{2}$ hours; at the end of this time the tube was opened and the brush removed. It now weighed 32·5613, showing a loss of 2·9327 grs. When heated

below redness the deposited film of gold was easily removed from the walls of the tube in the form of very brilliant foil.

After having been subjected to electrical volatilisation, the appearance of the residual piece of gold under the microscope, using a $\frac{1}{4}$-inch object glass, was very like that of electrolytically deposited metal, pitted all over with minute hollows.

This experiment on the volatilisation of gold having produced good coherent films of that metal, a similar experiment was tried, using a brush of platinum as a negative electrode. On referring to the table it will be seen that the electrical volatility of platinum is much lower than that of gold, but it was thought that by taking longer time a sufficient quantity might be volatilised to enable it to be removed from the tube.

The vacuum tube was exhausted to such a point as to give a dark space of 6 mm., and it was found, as in the case of gold, that as a coating of metal was deposited upon the glass the resistance rapidly increased, but in a much more marked degree, the residual gas in the tube apparently becoming absorbed as the deposition proceeded. It was necessary to let a little air into the tube about every 30 minutes, to reduce the vacuum. This appears to show that the platinum was being deposited in a porous spongy form, with great power of occluding the residual gas.

Heating the tube when it had become in this way non-conducting liberated sufficient gas to depress the gauge of the pump 1 mm., and to reduce the vacuum so as to give a dark space of about 3 mm. This gas was not re-absorbed on cooling, but on passing the current for ten minutes the tube again refused to conduct, owing to absorption. The tube was again heated, with another liberation of gas, but much less than before, and this time the whole was re-absorbed on cooling.

The current was kept on this tube for 25 hours; it was then opened, but I could not remove the deposited metal except in small pieces, as it was brittle and porous. Weighing the brush that had formed the negative pole gave the following results :—

	Grains.
Weight of platinum before experiment......	10·1940
,, after experiment	8·1570
Loss by volatilisation in 25 hours	2·0370

Another experiment was made similar to that with gold and platinum, but using silver as the negative pole, the pure metal being formed into a brush of fine wires. Less gas was occluded during the progress of this experiment than in the case of platinum. The silver behaved the same as gold, the metal deposited freely, and the vacuum was easily kept at a dark space of 6 mm. by the very occasional admis-

sion of a trace of air. In 20 hours nearly 3 grs. of silver were volatilised. The deposit of silver was detached without difficulty from the glass in the form of brilliant foil.

III. "A Study of the Planté Lead-Sulphuric Acid-Lead Peroxide Cell, from a Chemical Stand-point. Part I." By G. H. ROBERTSON. Communicated by Professor ARMSTRONG, F.R.S. Received May 27, 1891.

(Abstract.)

The author, in the introduction, states that though, since Frankland in 1883 published his first "Contribution to the Chemistry of Storage Batteries," the capabilities of the Planté cell have been well tested and are now thoroughly understood, there is still considerable uncertainty as to the precise nature of the chemical changes which attend their use; and that it was principally to study the part played by the electrolyte that the investigation, the results of which are recorded in this paper, was instituted about a year ago at the Central Institution at Dr. Armstrong's suggestion, as McLeod's observations on the electrolysis of sulphuric acid solutions led to the supposition that the changes occurring in the acid were probably less simple than was commonly supposed. This supposition was verified.

The first section of the paper deals with the nature of the lead salt formed during discharge. It is pointed out that, as is well known, red lead varies considerably in composition, generally containing a smaller proportion of peroxide than is represented by the formula $Pb_3O_4 = PbO_22PbO$; and that with nitric acid it behaves as though it were a mixture of the two oxides, the nitric acid always dissolving out the monoxide. There is no reason why sulphuric acid should not behave similarly, and, since lead sulphate is but very slightly soluble, red lead may be expected always to yield a corresponding sulphate, *i.e.*, a mixture of peroxide and sulphate, containing an amount of sulphate corresponding to the amount of monoxide originally present in combination with the peroxide. At Dr. Armstrong's request a number of experiments were made at the Central Institution (long prior to the reading of Messrs. Gladstone and Hibbert's papers) by two students, Messrs. Briggs and Ingold, on various samples of red lead, with the result that the sulphate formed always corresponded to the monoxide originally present.

As no proof of the existence of a definite homogeneous sulphate corresponding to red lead can be afforded by analysis alone, evidence must be obtained that the product differs in some of its properties from a mixture.

It was to be expected that the E.M.F. of an oxysulphate would differ from that of a corresponding mixture of sulphate and peroxide, and have some definite value; therefore pastes were made of peroxide of lead, peroxide of lead and sulphate in the proportions of 1 to 1 and 1 to 2, and also from the product obtained by treating red lead with dilute sulphuric acid (1 to 5). Experiments made with these pastes showed that there was a difference of degree only between the red lead pastes and the mixtures, and that the lowering of the E.M.F. appeared to depend rather on the intimacy of the mixture, and consequent thorough coating of the peroxide granules with sulphate, than on the proportion of sulphate present.

Mr. Desmond Fitzgerald had already shown, in 1887, at the Institution of Electrical Engineers, that the mere admixture of lead sulphate with peroxide of lead produces a lowering of the E.M.F.

With regard to Frankland's observations respecting the colour of the product formed on the peroxide plate during discharge and the reducibility of the sulphate, the author points out that the colour is due to the incomplete reduction of the peroxide, owing probably to the almost complete blocking up by lead sulphate of the pathways by which the current travels through the electrolyte in the paste; that careful examination of the plugs from a discharged cell shows that the base consists of practically unaltered peroxide of lead, and that the surface, which is rich in $PbSO_4$, is really a mass of partially reduced granules of peroxide of lead which are coated with sulphate.

Also, though pure lead sulphate is very difficult to reduce, it is very well known that mixtures of lead sulphate and peroxide of lead, or other conducting substances, are reduced with comparative ease, and it is very intimate mixtures of this nature which have to be dealt with as a rule in charging a cell.

In conclusion, the author points out:—

That neither chemical nor electrical tests give any ground for supposing that any other sulphate than the ordinary white $PbSO_4$ is concerned in the interactions occurring in the cell;

That were the sudden lowering of the E.M.F. caused by a change in the nature of the chemical compounds formed on the plates, it is very difficult to account for the very rapid recovery of the E.M.F. exhibited by an apparently discharged cell.

In the second section the electrolyte is dealt with, and, after referring to the work of Berthelot, Richarz, Schöne, Traube, and others on the electrolysis of sulphuric acid solutious, the author describes experiments made to test the effect of the addition of sodium sulphate to the electrolyte, as, from information received from Mr. Barber Starkey, it seemed probable that the different behaviour of cells containing sodium sulphate was due to the catalytic action of this salt on the hydrogen dioxide always found in electrolysed acid of the

strength used in batteries, and which is probably formed by secondary action from persulphuric acid.

Mr. Preece most kindly aided the investigation by allowing experiments to be carried out at the General Post Office, where one-half of the secondary cells contain 1 per cent. of sodium sulphate and the other half ordinary dilute acid, sp. gr. 1180. He also put at the disposal of the author the records of the behaviour of the cells, and as they showed that there was much less sulphating with sodium sulphate, as shown by the sp. gr. never falling to the same extent as in the plain cells, but that the general character of the changes in temperature and in sp. gr. during charge and discharge were the same in both types of cell, and of the nature which the work of Professor Ayrton and others has rendered familiar to all, it was only the distribution of temperature and of sp. gr. which was investigated, and this was found to be very irregular.

It was found that the addition of sodium sulphate, in about the proportion of 1 per cent., to freshly electrolysed acid, or during electrolysis, always produced a diminution in the total quantity of "active oxygen," and brought the amount present in the plain cells down almost exactly to that found in the sodium sulphate cells.

Experiments on the growth of the "peroxides" were carried on during five charges and discharges, but only the figures relating to the first discharge are given, as the other charges and discharges are mere repetitions. The peroxides form at once, then undergo a diminution, and then increase again. The alteration in the totals is due mainly to actions occurring at the lead plate in the plain cell, as at the peroxide plate the amounts steadily increase; in the sodium sulphate electrolyte there is a diminution at both plates, followed by an increase.

Determinations were made of the amounts of "active oxygen" present as persulphuric acid and hydrogen dioxide respectively, and it was established that acid taken from the cell reduced peroxide of lead. The presence of hydrogen dioxide being thus established both directly and indirectly, its effect on the E.M.F. of a cell was tested. It was found that, while its addition to the acid in the case of a lead peroxide couple in dilute sulphuric acid produced an annulment or reversal of the E.M.F., the introduction of hydrogen dioxide into the body of the peroxide paste produced an increase of E.M.F. in the case of a platinum-lead peroxide couple.

The latter experiment was made with a view of reproducing, if possible, the conditions of a cell which is started discharging directly it is fully charged, and in which the persulphuric acid formed at the positive during charge may be supposed to break up with the formation of hydrogen dioxide on the cessation of the charging current, thereby increasing the normal E.M.F. of the cell.

The cause of the pink colour of the acid noticed by Mr. Crompton and others was investigated, and found to be permanganic acid, formed probably from the manganese present in commercial lead.

In conclusion, the author points out:—

That "peroxides" are found in appreciable quantities in the electrolyte during charge and discharge;

That their influence must not be neglected in considering the behaviour of the Planté cell;

And that it is to the electrolyte rather than to the plates that attention must be directed if any considerable improvement is to be effected.

IV. "A Study of the Planté Lead-Sulphuric Acid-Lead Peroxide Cell, from a Chemical Stand-point. Part II.—A Discussion of the Chemical Changes occurring in the Cell." By H. E. Armstrong, F.R.S., and G. H. Robertson. Received June 4, 1891.

(Abstract.)

The authors arrive in this paper at the following conclusions:—

1. That the cooling observed in the Planté cell can only be explained as resulting from the dissociation of the dilute sulphuric acid; and as the values given by Messrs. Ayrton, Lamb, Smith, and Woods are in practical agreement with those calculated on the assumption that the acid used is sulphuric acid itself, H_2SO_4, that in all probability such acid and not the dilute acid contained in the cell is operative throughout.

2. That the observed loss in efficiency cannot be due to temperature changes, as these arise through actions occurring out of circuit.

3. That it is difficult from a comparison of calculated with observed values of the E.M.F. to arrive at any final conclusion as to the exact nature of the changes which take place in the cell. On the assumption that sulphating occurs at both plates in circuit and under the influence of H_2SO_4, the calculated value is considerably too high; while, if sulphating occur only at the lead plate, the value calculated is far too low.

4. That a counter E.M.F. of about 0·5 volt would account for the observed departure from the highest calculated value. As peroxides are always present in the electrolyte, it is conceivable that such a counter E.M.F. may exist; moreover, there is also the possible influence of the lead support to be considered.

5. That the observed loss of efficiency is to be attributed to the formation of peroxides in the electrolyte, and to the excessive sulphating occurring chiefly at the peroxide plate in the local circuit existing between the support and the paste.

V. "On the Influence of Temperature upon the Magnetisation of Iron and other Magnetic Substances." By HENRY WILDE, F.R.S. Received May 8, 1891.

In my paper on the "Unsymmetrical Distribution of Terrestrial Magnetism,"* it was shown that by heating small surfaces of the thin sheet iron entirely covering the ocean areas of the mapped globe strong magnetic polarity was induced at the heated parts, just as when the magnetic continuity of the iron was interrupted by cutting through the same parts of the iron in an equatorial direction. Although this experiment appeared to me to demonstrate conclusively that the magnetic power of iron was reduced by heating at comparatively low temperatures, and with small magnetising forces, yet, from the contradictory results which have been obtained by other experimenters on the magnetisation of heated iron, directly opposite conclusions as to the magnetic intensities of the land and ocean areas respectively might, with some reason, be drawn from those which I had arrived at.

Barlow, in an interesting paper on the magnetic behaviour of heated iron,† refers to the discordant opinions which prevailed on this subject among natural philosophers from the 17th century to his time, and assigned the cause of these discordances to the observations being made with iron at different degrees of heat.

Barlow found that the magnetic power of the bars of iron which he experimented upon, as measured by the deflections of a compass needle, *increased* with the temperature up to a dull red heat, at which it was the strongest; but, at a bright red heat, all magnetic action of the iron suddenly disappeared. Scoresby,‡ Christie,§ and others had also noted a similar increase in the magnetic power of iron with increase of temperature, when measured by the same means.

Faraday, on the other hand, has described experiments to show that the magnetic power of iron *diminishes* with increase of temperature.‖ He also found that iron at a bright red heat was not entirely insensible to the action of large magnetising forces.

More recently, Rowland,¶ Baur,** and Hopkinson,†† by the employment of electro-dynamic methods, have also found an increase in

* 'Roy. Soc. Proc.,' January 22, 1891.

† 'Phil. Trans.,' 1822, p. 117, &c.

‡ 'Edinburgh Roy. Soc. Trans.,' vol. 9, Part I.

§ Christie on Effects of Temperature, &c., 'Phil. Trans.,' 1825, p. 62, &c.

‖ 'Phil. Mag.,' 1836, vol. 8, p. 177; 'Phil. Trans.,' 1846, p. 41.

¶ 'Phil. Mag.,' 1874, vol. 48, p. 321.

** 'Wiedemann, Annalen,' vol. 11, 1880, p. 403.

†† 'Phil. Trans.,' A, 1889, vol. 180, p. 443.

the magnetic power of iron with increase of temperature. These experimenters were, however, the first to recognise that the apparent increase of the magnetic power of iron, up to the dull red heat, only held good for small magnetising forces, and, further, that the power diminished for large magnetising forces with ascending temperatures.

Rowland extended his observations to the magnetisation of nickel and cobalt, and found that the magnetic behaviour of these metals with increase of temperature was the same as that observed in iron.

Experiments have also recently been made by Professor Rücker* on the effects of temperature on the natural magnet (*magnetite*), and he has found, by means of an extremely sensitive instrument, that the magnetic power of this mineral increases as the temperature rises, as in the case of iron.

The important bearing which the influence of temperature has upon the phenomena of terrestrial magnetism induced me to undertake an investigation into the causes of the conflicting results obtained by those physicists who have preceded me in this research, with the hope also that I might be able to extend still further our knowledge of magnetic substances, especially in their relation to terrestrial physics.

The apparatus used in the investigation consisted of a bar electro-magnet, formed of a cylinder of iron 24 inches in length and 3·5 inches in diameter. The electro-magnet was placed in a vertical position, with its lower end screwed firmly into a massive base of cast iron. The upper end of the core was furnished with a short cylinder of iron, of the same diameter as the core, and having a conoidal termination, which constituted the pole of the magnet.

The magnetometer was a plain cylindrical needle, 4 inches long and 0·13 inch in diameter, suspended from a single fibre of untwisted silk. The needle received a charge of magnetism sufficient to support fourteen times its own weight from either pole, and was thickly covered with spun silk to prevent the weakening of its magnetism by close proximity to the heated substances under examination.

The iron experimented upon was a cylindrical bar of good malleable iron, 6 inches long and 0·7 inch in diameter. One end of the iron bar was drilled through its diameter to receive a strong iron pin, which projected crosswise on each side of the bar for the purpose of dropping it readily, when heated, into a stirrup placed over the electro-magnet. Several of these bars were prepared from the same rod of iron, to replace those which were reduced in thickness by fusion and oxidation, as well as for other experiments.

* 'Roy. Soc. Proc.,' 1890, vol. 48, p. 522.

The iron bar, with its stirrup, was pendent from the end of a balanced lever placed over the pole of the electro-magnet, while the arm of the lever, on the other side of the fulcrum, was weighted with a sliding weight, or with variable weights, to balance the attractive force of the iron when in contact with the electro-magnet.

For the measurement of smaller magnetic forces, a special balance was constructed which, besides balancing forces up to 15 lbs., would turn with a weight of less than half a grain.

Preliminary experiments were made upon the bar by placing it, when cold, in the direction of the dip, with one of its ends at a definite distance from the magnetometer, and in the same horizontal plane. When in this position, the magnetic force of the iron bar, augmented by the earth's magnetism, produced a deflection of the needle from the magnetic meridian of 20°.

The bar was then heated to bright redness, and replaced in the same position as before, when all the phenomena described by Barlow were reproduced. The heated bar had no perceptible action on the magnetometer, but on cooling down to a less red heat the magnetic action of the iron began to manifest itself, gradually at first, and then very rapidly, till the deflection of the needle, which was 20° with the cold iron, now advanced to 43°, thereby showing an increase of the magnetic power of the iron at this temperature. On the further cooling of the bar, the magnetic action of the iron gradually diminished, till the same deflection of the needle was obtained as at the commencement of the experiments.

The increase in the magnetism of the bar, as shown by this experiment, although greatly augmented by the earth's magnetism, was, however, very feeble; for when the bar was placed horizontally at the same distance from the needle the deflection was only increased 5° by heating it to the temperature most favourable to its magnetisation; and no increase of magnetism could be perceived when a small electro-magnet, however feebly excited, was brought into direct contact with the heated iron.

The bar was again heated to incipient whiteness, and placed over the pole of the electro-magnet. As the cooling proceeded, observations were taken of the intervals of time required before the magnetic force was sufficient to cause the bar to adhere to the pole of the electro-magnet for each definite increase of weight on the lever.

The colour, and several shades of colour, of the heated iron above visible redness, progressing towards the orange and yellow, are expressed in wave-lengths of well-known spectral lines of the alkaline and alkaline earth metals in the arc spectrum, as observed through a direct-vision spectroscope of five prisms. Below these temperatures, I have selected the melting points of zinc (442° C.) and tin (230° C.), small fragments of which metals could be dropped

into a cavity formed in the upper end of the bar. These temperatures were afterwards verified, and similar results obtained, when the bar was plunged into crucibles of the melted metals. The temperature of 100° C. was determined by plunging the bar into boiling water during the period of cooling. For temperatures below zero, a bath of solid carbonic acid and ether was employed, into which the bar was placed until it was cooled down to −76° C. The refrigerating arrangement was so effective, from the large supply of solid carbonic acid at my disposal, that a globule of mercury placed in the cavity at the upper end of the bar remained solid several minutes after the completion of the experiment.

All the experiments detailed in the following table were made with descending temperatures, as strictly concordant results were not obtained with definite increments of heat, especially for the lower ranges.

The electro-magnet was excited by a constant current of 20 ampères.

Table I.

Temperatures of bar.	Tractive force.	Times of cooling.	
λ	lbs.	m.	s.
Yellow, Na 5895	0·002	0	00
Orange, Ba 6141	0·008	0	13
Red, Ba 6496	1·0	0	20
,,	6·0	0	11
,, Li 6705	12·0	0	8
,,	18·0	0	14
,, Ka 6946	24·0	0	22
,,	30·0	0	29
,, Rb 7800	36·0	0	49
+442° C.	42·0	1	25
+230° C.	47·0	6	44
+100° C.	50·0	8	20
+ 13° C.	52·0	No observation.	
− 76° C.	53·6	,,	
Time of cooling from λ 5895 to 100° C.		19	15

The principal result shown by these experiments is the continued diminution of the magnetic power of the iron, from the lowest to the highest temperature to which the bar was subjected.

As it was of importance to determine whether iron entirely loses its magnetic power by heating, the temperature of one of the bars was raised to incipient fusion; but when the bar was carefully balanced there still remained in it a measurable amount of magnetic force when the electro-magnet was brought into action.

The results also show a rapid increase of the magnetic power of the bar from λ 6496 to λ 6705. This increase was attributed, in the first instance, to an error of observation, but on repeating the experiment similar results were obtained. The rapid increment of magnetic force in the interval of cooling between these spectral lines may, therefore, be regarded as a real phenomenon.

In all the experiments which have hitherto been made, where an increase of the magnetic power of iron with increase of temperature has been observed, it does not appear to have been suspected that the *mass* of the iron, in relation to the magnetising forces employed, might be an important factor in the results obtained, and that small magnetising forces might only penetrate to a small depth below the surface of the iron, when cold, till the more central portions of the mass were brought into action by increase of temperature. Several magnetic properties of iron and steel, however, point to the probability of this action of weak magnetising forces. Coulomb found that the magnetism of similar steel bars did not increase in the ratio of their number when laid together, from which it was inferred that the magnetism diminished from the surface to the centre of the bars. Joule has shown that a hollow electro-magnet has greater attractive force than a solid one of the same sectional area, with a small magnetising current.* It is also well known that the distribution of magnetism on the polar surfaces of electro-magnets is much greater at the circumference than it is at the centre.

That small magnetising forces penetrate but a small depth into a mass of iron was shown by heating one of the bars to redness, and sprinkling over its surface ferrocyanide of potassium, in powder, before plunging the heated bar into cold water. The conversion of the surface of the iron into steel by this well known process was sufficient to reduce the deflection of the needle from 20° to 15°, when the bar was placed in the direction of the dip.

In addition to the evidence adduced of the surface action of small magnetising forces on a mass of iron, it further appeared to me that as the time and limit of magnetisation of iron vary with the mass, for constant magnetising currents, or that the time and limit of magnetisation are constant when the magnetising current and mass vary in proportion, so it also appeared to me that, when the mass of iron and magnetising force were proportional, the diminution of the magnetic power of iron with increase of temperature would be constant for small, as well as for large, magnetising forces.

That the increase of the magnetic power of the heated bar, as shown by the magnetometer, was caused by the large mass of the iron in relation to the magnetising force of the needle was shown by the following experiments:—

* 'Annals of Electricity,' 1840, vol. 4, p. 60.

(*a*.) A small cylinder of iron wire, 0·2 inch long and 0·5 inch in diameter, was mounted in a twisted loop formed at the end of a piece of thin copper wire. The copper wire was fixed to an arm moving horizontally, in such manner that, when the cylinder of iron was brought into close proximity to one pole of the needle, the iron drew the needle from the magnetic meridian to a point where equilibrium was established between the attractive force of the iron and the earth's magnetism. The needle was then blocked a small fraction of an inch in advance and also behind this position; so that any increase or diminution of the magnetic power of the iron would limit the range of the needle to a fraction of an inch in either direction.

When a small gas-flame, or a lighted taper, was brought under the cylinder of iron wire till it became visibly red hot, the needle receded from the iron towards the magnetic meridian, thereby indicating a diminution of the magnetic power of the iron by the magnetometer which had previously shown an increase in the magnetic action of the large bar. On removing the source of heat from the iron, the needle again advanced towards it. On reheating the iron, the needle again receded, and the operation could be repeated at pleasure.

[That the recession of the needle from the heated iron was not due to the temperature being sufficiently high to render the iron virtually non-magnetic was shown by the needle again advancing towards the heated iron when brought into closer proximity to it without change of temperature, but no increase was observable in the magnetic power of the iron with any increase of temperature above 13° C.—*June* 3, 1891.]

(*b*.) A small cylinder of steel was prepared from the same piece as that from which the magnetometer needle was cut, and of the same dimensions as the one used in the previous experiment. All trace of permanent magnetism was removed from the steel by heating it to bright redness in a blowpipe flame, so that either end of the cylinder when cold was attracted indifferently by the same pole of the needle. When the steel was submitted to the process of heating and cooling in proximity to the magnetometer, as in experiment (*a*), its magnetic behaviour was the same as that observed with the iron.

The increase of magnetic power in the iron and steel, during the period of cooling, appeared to come on gradually, in the same manner as the magnetic power of the bar in relation to the large electro-magnet; and both experiments show decisively that the magnetic power of iron diminishes with increase of temperature for small, as well as for large, magnetising forces.

(*c*.) A fragment of the natural magnet (*magnetite*), weighing 2 grains, was detached from a compact and well crystallised mass of this mineral. The magnetite was heated to bright redness to remove all

trace of polarity, and care was taken to prevent new polarities being given to it by accidental contact with the needle. On heating the magnetite in the wire loop, as in experiment (*a*), the influence of temperature was more marked than with the iron, as the needle receded towards the magnetic meridian before the magnetite was visibly red-hot, and advanced again very readily when the source of heat was removed.

(*d.*) A small rectangular prism of nickel, 0·2 inch long and 0·05 inch across the sides, was submitted to the magnetometer as in the previous experiments, when the increase of temperature in diminishing the magnetic power of the nickel was most pronounced at the temperature of melted tin, and the metal became quite insensible to the needle at a point much below the red heat.

(*e.*) A rectangular prism of pure cobalt, of the same dimensions as that used in the previous experiment, was submitted to the action of the magnetometer, when, contrary to expectation, the needle advanced towards the cobalt before it became visibly red-hot, and remained stationary when the temperature was raised to redness; thereby showing an increase of magnetic power of the cobalt with increase of temperature.

The magnetic behaviour of the cobalt was so remarkable as to induce me to make further experiments upon the metal with more powerful magnetising forces.

A cube of pure cobalt from the Chemical Museum of the Owens College was kindly placed at my disposal for these experiments by Professor H. B. Dixon, F.R.S. The cube was 0·3 inch across the sides, and a short piece of platinum was screwed into the centre of one of its faces for suspension from the balance over the large electro-magnet. The temperature of the cube, below the red heat, was determined by the fusion of small fragments of zinc and tin, placed in a conical recess drilled into the upper face of the cube.

Similar cubes of nickel and malleable iron were prepared for comparison with the results obtained with the cobalt.

The method of experimenting was as follows:—The cube of magnetic metal was suspended over the electro-magnet, excited by a current of 20 ampères, and while in this position was heated by an oxyhydrogen flame until the requisite temperature was attained. The cube was then quickly brought into contact with the pole of the electro-magnet, without any intermission of the heating blast, and the magnetic force was measured by the weight required to detach the cube from the electro-magnet. The cube was reduced to the temperature of $-76°$ C. by immersing it in a bath of solid carbonic acid and ether, whilst suspended over the electro-magnet.

The results of these experiments with the magnetic metals are given below:—

Table II.

Temperatures.	Tractive force in lbs.		
	Iron.	Nickel.	Cobalt.
Orange, Ba 6141	0·0005	0·0002	0·02
Red, Ba 6496	0·02	0·0003	5·00
„ Rb 7800	9·50	0·001	6·31
+ 442° C.	11·00	0·024	7·25
+ 230° C.	12·50	2·000	6·75
+ 13° C.	12·75	3·125	6·31
− 76° C.	12·87	3·312	6·12

The principal feature of interest in the table is the same inversion of the magnetic power of the heated cube of cobalt, in relation to iron and nickel, as was obtained by the minute force of the magnetometer needle acting upon the small prism of the same metal. The increase of the magnetic power of the heated cube was, however, much greater relatively with smaller magnetising forces; for, while the ratio of increase with 20 ampères of current was as 1 : 1·15 between 13° C. and 442° C., the ratio with 3 ampères was as 1 : 1·6 between the same temperatures.

The abruptness of the change in the magnetic condition of iron, nickel, and cobalt, observed by Faraday,* at what is now aptly termed the critical temperature, is also well seen in the table.

Following up the results of the experiments which showed that the apparent increase in the magnetic power of heated iron was dependent upon the mass in relation to the magnetising force, it appeared to me that heated cobalt might show a diminution of magnetic power, as in the case of iron and nickel, if a sufficiently large magnetising force were brought to bear upon a minute quantity of the metal, notwithstanding that it had so far shown an increase of power for large, as well as for small, magnetising forces.

A minute cylinder of cobalt, 0·06 inch long, 0·05 inch in diameter, and ¼ grain in weight, was formed from a piece of the same cube of the metal used in the previous experiments. A small hole was drilled up the end of a thick piece of copper wire in the direction of its axis, into which the cylinder of cobalt was driven tightly for nearly the whole of its length. An eye was formed at the other end of the copper wire for suspending the cobalt over the electromagnet.

Similar cylinders of iron and nickel were formed from the cubes

* 'Phil. Mag.,' 1836, vol. 8, p. 177; *ibid.*, 1845, vol. 27, p. 1.

experimented with, and mounted for suspension over the electro-magnet in the same manner as the cylinder of cobalt.

The flat end of the electro-magnet was surmounted by a cone of iron 4 inches high and 3 inches in diameter at the base, with the apex rounded to form a pole 0·1 inch in diameter.

As a test of the magnetic intensity at the pole of the electro-magnet, the little cylinder of iron was suspended from the balance, when the tractive force was 0·601 lb. with 20 ampères of current, which is equal to 305 lbs. per square inch of section, or more than 17,000 times the weight of the iron.

When the cobalt was submitted to the same magnetising force as the iron, the tractive force at 13° C. was 0·304 lb., which is equal to 154 lbs. per square inch of section, or 8000 times the weight of the cylinder of cobalt.

On heating the cobalt, whilst suspended over the electro-magnet, a constant diminution of the magnetic power of the metal was now observed from 13° C., as in the case of iron and nickel, the tractive force diminishing from 0·304 lb. at 13° C. to 0·296 lb. at 442° C.

The results of the experiments with the minute cylinders of the magnetic metals are given below :—

Table III.

Temperatures.	Tractive force with current = 5 ampères.	Tractive force with current = 20 ampères.	Tractive force per sq. in. with current = 20 ampères.	Ratio of tractive force to weight of metals.
	lb.	lb.	lbs.	
Iron—				
442° C.	0·390	0·547		
13° C.	0·437	0·601	305	17000
Nickel—				
442° C.	0·001	0·003		
13° C.	0·064	0·127	64	3300
Cobalt—				
Ba 6496	0·109	0·172		
442° C.	0·156	0·296		
13° C.	0·140	0·304	154	8000

That the property of the anomalous increase of the magnetic power of the heated cobalt was broken down by the intensity of the magnetic force and the diminution of the mass conjointly, as in the case of the small and large bars of iron, was further shown by submitting the little cylinder of cobalt to the action of the electro-magnet excited by 5 ampères of current, when the tractive force of the heated cobalt was increased from 0·140 lb. at 13° C. to 0·156 lb. at 442° C.

On comparing the tractive force of the cobalt with that of iron, each with 5 ampères of current, it will be seen from the table that it was still very high, being no less, for the iron, than 0·437 lb. = 222 lbs. per square inch of section, or more than 12,000 times its own weight. Although this amount of tractive force is greater than any so far recorded for iron, yet the magnetising force was not sufficient to break down the property of the increase of magnetic force of the heated cobalt. It is well, however, that I should point out that the property only pertains in the highest degree to the metal when in a state of purity, as several of the specimens experimented upon, from different sources, only exhibit the property in a feeble manner, the diminution being due to the presence of iron in the cobalt.

As the determination of the limit of the magnetisability of iron by different methods is of some importance to magnetical science, an experiment was made on a cylinder of annealed charcoal iron wire 0·2 inch long, 0·05 inch in diameter, and $\frac{3}{4}$ grain in weight. The cylinder was driven up the end of a thick copper wire for the purpose of suspension as in the previous experiments. The tractive force of this specimen of iron at 13° C., with 40 ampères of current, was 0·75 lb., which is equal to 381 lbs. per square inch of section, or 7000 times the weight of the iron.

That the limit of magnetisability was virtually arrived at in this experiment was shown by reducing the current to 20 ampères, when the tractive force remained at 0·734 lb. = 373 lbs. per square inch of section, or only 8 lbs. less per square inch than the tractive force obtained with 40 ampères of current.

Presents, June 11, 1891.

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June 18, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Professor Daniel John Cunningham, Professor Percy Faraday Frankland, and Mr. William Napier Shaw were admitted into the Society.

The following Papers were read:—

I. "Results of Hemisection of the Spinal Cord in Monkeys." By FREDERICK W. MOTT, M.D., B.S., M.R.C.P. Communicated by Professor SCHÄFER, F.R.S. Received June 1, 1891.

(Abstract.)

While engaged in studying experimentally the connexions of the cells of Clarke's column with the ascending tracts of the spinal cord in the monkey, I was surprised to find that after hemisection in the lower dorsal region the sensory disturbances produced in no way corresponded with those already obtained by eminent observers.

I was, therefore, led to continue my experiments, and, by the kind permission of Professor Schäfer, I carried them out in the Physiological Laboratory of University College. My thanks are also due to him for much valuable advice and assistance.

The subject is one of great importance from a scientific, as well as from a clinical, point of view. Some years ago, a case occurred in my practice which tended to shake my faith in the absolute truth of the doctrine of complete and immediate decussation of sensory impulses in the spinal cord, as taught by Brown-Séquard.

The experiments which I have performed exhibit the following principal points of interest:—

1. Return of associated movements after complete destruction of the crossed pyramidal tract below the lesion.

2. That all sensory impulses do not decussate in the cord, in fact, they appear to show that certain sensory impulses, *e.g.*, touch, the muscular sense and localisation in space, pass chiefly up the same side, painful impressions up both sides. A peculiar condition known as "allochiria" occurs after hemisection.

3. The vaso-motor disturbances are on the same side as the lesion, and consist of vaso-dilation, swelling of the foot, and redness with rise of temperature of the skin of the foot, but, as compared with the opposite side, fall of temperature in the popliteal space on the side of the lesion, due, no doubt, to paralysis of the muscles.

4. The degenerations above and below the lesion are limited to the same side when the injury is perfectly unilateral. There are certain facts connected with the degenerations which serve to show the origin and course of certain long and short tract fibres.

5. Stimulation of the cortex cerebri on both sides some weeks or months after the hemisection had been performed gave, as a rule, *results* which showed that the block in the spinal cord produced by the hemisection still existed, although there had been a very complete return of associated movements.

6. In one case ablation of the leg area on the same side as the lesion in the spinal cord was performed many months afterwards.

II. "The Origin and Progressive Motions of Cyclones in the Western India Region." By W. L. DALLAS. Communicated by R. H. SCOTT, F.R.S. Received June 2, 1891.

III. "Note on the Density of Alloys of Nickel and Iron." By J. HOPKINSON, F.R.S. Received June 3, 1891.

In the 'Proceedings of the Royal Society,' December 12, 1889, January 16, 1890, and May 1, 1890, I described certain properties of alloys of nickel and iron containing respectively 22 per cent. and 25 per cent. of nickel. These alloys can exist in two states at temperatures between 20° or 30° C. below freezing and a temperature of near 600° C. After cooling, the alloys are magnetisable, have a lower electric resistance, a higher breaking stress, and lesser elongation; after heating the alloys are not magnetisable, have a higher electric resistance, a lower breaking stress, and greater elongation. I have now to add another curious property. These alloys are about 2 per cent. less dense when in the magnetisable than when in the non-magnetisable state. Two rings were tested containing respectively 25 per cent. and 22 per cent. of nickel with the following results, the densities being given without correction in relation to the density of water at the then temperature:—

	Nickel, 25 per cent.		Nickel, 22 per cent.	
	Density.	Temp.	Density.	Temp.
After heating, non-magnetisable	8·15	15·1	8·13	16·5
After cooling, magnetisable	7·99	14·5	7·96	15·6
After heating again, non-magnetisable	8·15	18·0	8·12	18·2
After cooling again, magnetisable	7·97	22·0	7·95	21·8

The rings were each time cooled to from −100° C. to −110° C. by carbonic acid and ether *in vacuo.*

IV. "An Apparatus for testing the Sensitiveness of Safety-lamps." By FRANK CLOWES, D.Sc., Lond., Professor of Chemistry, University College, Nottingham. Communicated by Professor ARMSTRONG, F.R.S. Received June 4, 1891.

It is generally acknowledged that the Davy safety-lamp cannot with certainty detect less than 3 per cent. of firedamp in the air of the mine. Gas-indicators of much greater sensitiveness have been invented; amongt these the electrical apparatus of Liveing and the spirit safety-lamp of Pieler take first rank. The objection to these special forms is, however, a serious one. They do not serve for illuminating purposes, and therefore it becomes necessary to carry an ordinary safety-lamp, together with the testing apparatus. Many attempts have been made to obviate this inconvenience by producing a safety-lamp which shall serve the double purpose of illumination and of detecting minute percentages of firedamp. The discovery of such a lamp would be of great value to the miner, in view of the fact that very low percentages of firedamp have been proved to be dangerous in the presence of coal-dust.

The following apparatus has been devised to render easy the process of testing the sensitiveness of different forms of safety-lamps when used for detecting firedamp. To enable satisfactory tests to be made in the laboratory, it was necessary to insure (1) the easy and rapid production of mixtures of firedamp and air in known proportions; (2) to insure economy of the artificially prepared methane, which represented firedamp; and (3) to examine the flame of the lamp under conditions as satisfactory as those existing in the mine.

A wooden cubical box of about 100 litres capacity was constructed so as to be as nearly gas-tight as possible. It was then made absolutely gas-tight by painting it over with melted paraffin wax, which was afterwards caused to penetrate more perfectly by passing an ordinary hot flat-iron over the surface. This testing chamber was furnished with a small inlet tube at the top, and with a similar outlet

Fig. 1.

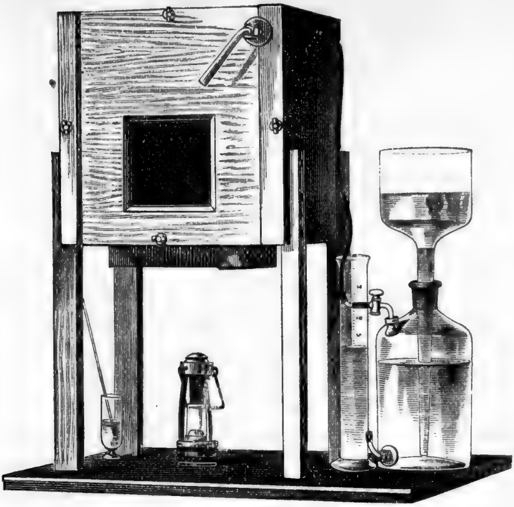
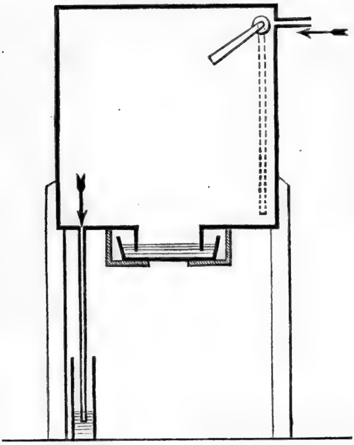


Fig. 2.



tube below. It had a plate-glass window in front for observing the lamp in the interior, and a flanged opening below for introducing the safety-lamp. This opening was closed by a water-seal consisting of a small zinc tray supported by buttons, and containing about 2 inches depth of water, into which the flange dipped. A mixer was arranged, which consisted of a light flat board, nearly equal in dimensions to the

section of the chamber, and suspended by an axis from the upper corner of the chamber. The mixer was moved rapidly backwards and forwards from the side to the top of the interior of the chamber, by grasping a handle projecting through the front of the chamber.

When a mixture of air with a certain definite percentage of fire-damp was required, the methane, prepared and purified by ordinary chemical methods, was introduced into the chamber in the requisite quantity by the top inlet. It displaced an equal volume of air, which escaped through the lower outlet, the exit end of which was sealed by being immersed just beneath a water surface. A vigorous use of the mixer secured a uniform mixture of gas and air throughout the interior of the chamber in the course of a few seconds. The lamp was then introduced into the chamber, and placed in position behind the glass window. The simplicity of arrangement of the water-seal rendered the necessary opening of the chamber very brief, and the introduction and removal of the lamp many times in succession was not found to produce any appreciable effect upon the composition of the atmosphere inside the chamber. The appearance and dimensions of the "cap" over the flame were noted as soon as the cap underwent no further change. A lamp was left burning in the chamber for a considerable length of time, and its indications underwent no change, owing to the large capacity of the chamber and the very limited amount of air required to support the combustion of the small flame always used in gas-testing. The whole interior of the chamber and mixer were painted dead-black, so as to render visible pale and small caps against a black ground.

The methane was introduced from an ordinary gas-holder. A volume of water, equal to that of the methane to be displaced, was poured into the top of the gas-holder. The gas-tap of the holder was then momentarily opened, so as to produce equilibrium of pressure between the methane and the atmosphere. The gas-tap having then been placed in connexion with the upper inlet of the chamber, the water-tap was opened, and the measured volume of water was allowed to flow down and drive the methane into the chamber. As soon as bubbles of air ceased to appear through the water at the outlet, the chamber was closed; the mixer was then vigorously worked for a few seconds, and the mixture of gas and air was ready for the introduction of the lamp. Before introducing the methane for a fresh mixture, the atmosphere of the chamber was replaced by fresh air by removing the water-tray from beneath the opening at the bottom of the chamber, and blowing in a powerful stream of air from a bellows to the top of the chamber.

The chamber was supported on legs, which were arranged so as to place it at a convenient height for observations through the window, and also for the introduction and removal of the safety-lamp.

The accuracy of this method was tested by introducing the Pieler lamp into the chamber, which was charged successively with a series of mixtures containing proportions of methane varying from 0·5 to 4 per cent. The height and appearance of the cap over the flame absolutely corresponded with a series of standard tests already published, and made by a different method, in which firedamp was used instead of methane.

The observations were usually made in a darkened room, but the flame-caps were easily seen in a lighted room, provided direct light falling on the eye or chamber was avoided.

The capacity of the chamber was 95,220 c.c.; accordingly the following volumes of methane were introduced: for $\frac{1}{2}$ per cent. mixture 476 c.c., for 1 per cent. 952 c.c., for 2 per cent. 1904 c.c., for 3 per cent. 2856 c.c., for 4 per cent. 3808 c.c., and for 5 per cent. 4760 c.c. It will be seen that a series of tests, in which the above-mentioned percentage mixtures were employed, involves an expenditure of only 15 litres of methane, a quantity far smaller than that required by any other method of testing as yet described.

Of many forms of safety-lamp tested in the above apparatus, the one which most satisfactorily fulfilled the two purposes of efficient illumination and delicacy in gas-testing was Ashworth's improved Hepplewhite-Gray lamp. This lamp is of special construction, burns benzoline from a sponge reservoir, and its flame is surrounded with a glass cylinder, which is ground rough at the hinder part; this latter device prevents the numerous reflected images of the flame, and the generally diffused reflections which are seen from a smooth glass surface, and which render the observation of a small pale flame-cap very difficult, if not impossible.

The wick of this lamp, when at a normal height, furnishes a flame of great illuminating power. When lowered by a fine screw adjustment the flame becomes blue and non-luminous, and does not interfere therefore with the easy observation of a pale cap. The following heights of flame-cap were observed, which fully bear out the unusual sensitiveness of this flame. With 0·5 per cent. of methane 7 mm.; with 1 per cent. 10 mm.; with 2 per cent. 14 mm.; with 3 per cent. 20 mm.; with 4 per cent. 25 mm.; and with 5 per cent. 30 mm. The cap, which with the lower proportions was somewhat ill-defined, became remarkably sharp and definite when 3 per cent. and upwards of methane was present. But even the lowest percentage gave a cap easily seen by an inexperienced observer.

It appears from the above record of tests that the problem of producing a lamp which shall serve both for efficient illuminating and for delicate gas-testing purposes has been solved. The solution is in some measure due to the substitution of benzoline for oil, since the

flame of an oil-flame cannot be altogether deprived of its yellow luminous tip, without serious risk of total extinction; and this faint luminosity is sufficient to prevent pale caps from being seen.

From further experiments made in the above testing chamber with flames produced by alcohol and by hydrogen, it was found to be true in practice, as might be inferred from theory, that, if the flame was pale and practically non-luminous, the size and definition of the flame-cap was augmented by increasing either the size or the temperature of the flame. It is quite possible by attending to these conditions to obtain a flame which, although it is very sensitive for low percentages of gas, becomes unsuitable for the measurement of any proportion of gas exceeding 3 per cent. This must, for the general purposes of the miner, be looked upon as a defect; but it is not a fault of the lamp already referred to. It is of interest to note that with the Pieler spirit-lamp a flame-cap an inch in height was seen in air containing only 0·5 per cent. of methane.

V. "On the Forces, Stresses, and Fluxes of Energy in the Electromagnetic Field." By Oliver Heaviside, F.R.S. Received June 9, 1891.

(Abstract.)

The abstract nature of this paper renders its adequate abstraction difficult. The principle of conservation of energy, when applied to a theory such as Maxwell's, which postulates the definite localisation of energy, takes a more special form, viz., that of the continuity of energy. Its general nature is discussed. The relativity of motion forbids us to go so far as to assume the objectivity of energy, and to identify energy, like matter; hence the expression of the principle is less precise than that of the continuity of matter (as in hydrodynamics), for all we can say in general is that the convergence of the flux of energy equals the rate of increase of the density of the energy; the flux of the energy being made up partly of the mere convection of energy by motion of the matter (or other medium) with which it is associated localisably, and partly of energy which is transferred through the medium in other ways, as by the activity of a stress, for example, not obviously (if at all) representable as the convection of energy. Gravitational energy is the chief difficulty in the way of the carrying out of the principle. It must come from the ether (for where else can it come from?), when it goes to matter; but we are entirely ignorant of the manner of its distribution and transference. But, whenever energy can be localised, the principle of continuity of energy is (in spite of certain drawbacks connected with the circuital flow of energy) a valuable principle which should be utilised to the

uttermost. Practical forms are considered. In the electromagnetic application the flux of energy has a four-fold make-up, viz., the Poynting flux of energy, which occurs whether the medium be stationary or moving; the flux of energy due to the activity of the electromagnetic stress when the medium is moving; the convection of electric and magnetic energy; and the convection of other energy associated with the working of the translational force due to the stress.

As Electro-magnetism swarms with vectors, the proper language for its expression and investigation is the Algebra of Vectors. An account is therefore given of the method employed by the author for some years past. The quaternionic basis is rejected, and the algebra is based upon a few definitions of notation merely. It may be regarded as Quaternions without quaternions, and simplified to the uttermost; or else as being merely a conveniently condensed expression of the Cartesian mathematics, understandable by all who are acquainted with Cartesian methods, and with which the vectorial algebra is made to harmonise. It is confidently recommended as a practical working system.

In continuation thereof, and preliminary to the examination of electromagnetic stresses, the theory of stresses of the general type, that is, rotational, is considered; and also the stress activity, and flux of energy, and its convergence and division into translational, rotational, and distortional parts; all of which, it is pointed out, may be associated with stored potential, kinetic, and wasted energy, at least so far as the mathematics is concerned.

The electromagnetic equations are then introduced, using them in the author's general forms, *i.e.*, an extended form of Maxwell's circuital law, defining electric current in terms of magnetic force, and a companion equation expressing the second circuital law; this method replacing Maxwell's in terms of the vector potential and the electrostatic potential, Maxwell's equations of propagation being found impossible to work and not sufficiently general. The equation of activity is then derived in as general a form as possible, including the effects of impressed forces and intrinsic magnetisation, for a stationary medium which may be eolotropic or not. Application of the principle of continuity of energy then immediately indicates that the flux of energy in the field is represented by the formula first discovered by Poynting. Next, the equation of activity for a moving medium is considered. It does not immediately indicate the flux of energy, and, in fact, several transformations are required before it is brought to a fully significant form, indicating (1) the Poynting flux, the form of which is settled; (2) the convection of electric and magnetic energy; (3) a flux of energy which, from the form in which the velocity of the medium enters, represents the flux of energy due

to a working stress. Like the Poynting flux, it contains vector products. From this flux the stress itself is derived, and the form of translational force, previously tentatively developed, is verified. It is assumed that the medium in its motion carries its properties with it unchanged.

A side matter which is discussed is the proper measure of "true" electric current, in accordance with the continuity of energy. It has a four-fold make-up, viz., the conduction current, displacement current, convection current (or moving electrification), and the curl of the motional magnetic force.

The stress is divisible into an electric and a magnetic stress. These are of the rotational type in eolotropic media. They do not agree with Maxwell's general stresses, though they work down to them in an isotropic homogeneous stationary medium not intrinsically magnetised or electrised, being then the well-known tensions in certain lines with equal lateral pressures.

Another and shorter derivation of the stress is then given, guided by the previous, without developing the expression for the flux of energy. Variations of the properties permittivity and inductivity with the strain can be allowed for. An investigation by Professor H. Hertz is referred to. His stress is not agreed with, and it is pointed out that the assumption by which it is obtained is equivalent to the existence of isotropy, so that its generality is destroyed. The obvious validity of the assumption on which the distortional activity of the stress is calculated is also questioned.

Another form of the stress vector is examined, showing its relation to the fictitious electrification and magnetic current, magnetification and electric current, produced on the boundary of a region by terminating the stress thereupon; and its relation to the theory of action at a distance between the respective matters and currents.

The stress subject is then considered statically. The problem is now perfectly indeterminate, in the absence of a complete experimental knowledge of the strains set up in bodies under electric and magnetic influence. Only the stress in the air outside magnets and conductors can be considered known. Any stress within them may be superadded, without any difference being made in the resultant forces and torques. Several stress formulæ are given, showing a transition from one extreme form to another. A simple example is worked out to illustrate the different ways in which Maxwell's stress and others explain the mechanical actions. Maxwell's stress, which involves a translational force on magnetised matter (even when only inductively magnetised), merely because it is magnetised, leads to a very complicated and unnatural way of explanation. It is argued, independently, that no stress formula should be allowed which indicates a translational force of the kind just mentioned.

Still the matter is left indeterminate from the statical standpoint. From the dynamical standpoint, however, we are led to a certain definite stress distribution, which is also, fortunately, free from the above objection, and is harmonised with the flux of energy. A peculiarity is the way the force on an intrinsic magnet is represented. It is not by force on its poles, nor on its interior, but on its sides, referring to a simple case of uniform longitudinal magnetisation; *i.e.*, it is done by a *quasi*-electromagnetic force on the fictitious electric current which would produce the same distribution of induction as the magnet does. There is also a force where the inductivity varies. This force on fictitious current harmonises with the conclusion previously arrived at by the author that, when impressed forces set up disturbances, such disturbances are determined by the curl of the impressed forces, and proceed from their localities.

In conclusion it is pointed out that the determinateness of the stress rests upon the assumed localisation of the energy and the two laws of circuitation, so that with other distributions of the energy (of the same proper total amounts) other results would follow; but the author has been unable to produce full harmony in any other way than that followed.

VI. "Comparison of Simultaneous Magnetic Disturbances at several Observatories, and Determination of the Value of the Gaussian Functions for those Observatories." By W. GRYLLS ADAMS, D.Sc., F.R.S., Professor of Natural Philosophy in King's College, London. Received June 11, 1891.

(Abstract.)

After drawing attention to previous investigations on this subject, and pointing out the importance of adopting the same scale values for similar instruments at different Observatories, especially at new Observatories which have been recently established, the discussion of special magnetic disturbances is undertaken, especially the disturbances of a great magnetic storm which occurred on June 24 and 25, 1885, for which photographic records have been obtained from 17 different Observatories: 11 in Europe, 1 in Canada, 1 in India, 1 in China, 1 in Java, 1 at Mauritius, and 1 at Melbourne.

The records are discussed and compared, tables are formed of the simultaneous disturbances, and the traces are reduced to Greenwich mean time and brought together on the same plates arranged on the same time-scale. Plates I and II show the remarkable agreement between the disturbances at the different Observatories, and the

Tables show that the amount of disturbance, especially of horizontal magnetic force, is nearly the same at widely distant stations.

An attempt has also been made to apply the Gaussian analysis to sudden magnetic disturbances, and, with a view to their application in future work, the values of the Gaussian functions have been obtained for 20 different Observatories, and the numerical equations formed for the elements of magnetic force in three directions mutually at right angles, and also the equation for the magnetic potential in terms of the Gaussian coefficients to the fourth order.

The Tables give the numerical values to be multiplied by the 24 Gaussian coefficients to give the values of the forces X, Y, and Z in the geographical meridian towards the north, perpendicular to the meridian towards the west, and vertically downwards respectively. The equations are also formed and the values obtained in terms of the 24 Gaussian coefficients for X_2, Y_2, and Z_2, X_2 being the horizontal force in the magnetic meridian, Y_2 the horizontal force perpendicular to the magnetic meridian, and Z_2 the vertical force. If then X_2, Y_2, and Z_2 be the observed values of any simultaneous disturbances, they may be at once substituted in the equations, the equations giving the 24 Gaussian coefficients may be solved, and the corresponding change of magnetic potential may be determined.

VII. "On the Measurement of the Heat produced by Compressing Liquids and Solids." By the late Cosmo Innes Burton, B.Sc., F.C.S., Professor of Chemistry, Polytechnic, Shanghai, and William Marshall, B.Sc., F.C.S. Communicated by Professor Thorpe, F.R.S. Received June 10, Read June 18, 1891.

In the year 1885 Messrs. Creelman and Crocket ('Edinburgh Roy. Soc. Proc.,' vol. 13, p. 311), under Professor Tait's supervision, performed a series of experiments on the heat produced by the compression of various substances. Their method was briefly as follows:—For the application of the pressure, the same apparatus which we describe and figure later was used. A thermo-electric junction of insulated nickel and iron wires was fixed between the leather washers and a sufficient length of wire coiled away inside the gun to allow the junction to be drawn out at the top and a specimen attached to it. Among the substances examined were glass, cork, vulcanite, glue, bees'-wax, and paraffin oil, the only pure chemical compounds being chloroform and ether. The following are some of the results obtained. Pressure, about 1 ton on the square inch.

	Rise of temp. per ton.
Vulcanite	$0^{\circ}\cdot30$
Glass	0·12
Cork	0·75
Beeswax	0·83
Chloroform	1·44
Ether	1·80
Paraffin (solid)	0·61
Paraffin oil	1·39

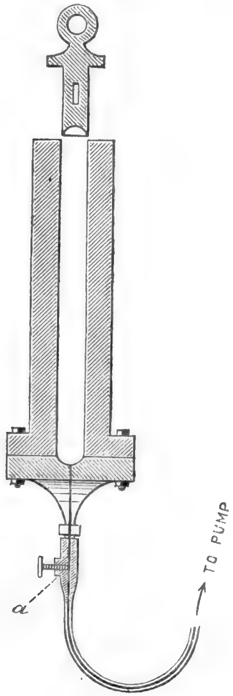
Owing to the unfortunate choice of substances, these results, although of interest as illustrating the application of the method, can have little, if any, general significance. In the year 1888 Mr. Burton performed a similar series of experiments, with the view of gaining some light on the physical constitution of allotropic forms of elementary substances. As the results have not hitherto been published, they are here shortly recorded. The following table gives a *résumé* of the results obtained with the two forms of phosphorus and with several metals. The method was in nearly all particulars the same as that of Messrs. Creelman and Crocket. The powdered substances were strongly compressed in short glass tubes, and the sharp-pointed nickel-iron junction pressed into the powder. The metals were made into little cylinders, and a hole drilled in each, very little larger than the junction. The sample was fixed on the wires and then finely powdered metal packed in, so as to leave the least possible air space. This method proved fairly satisfactory for such metals as could be obtained in ingots, and for very heavy and coherent powders, such as graphite, arsenic, and amorphous phosphorus; but it was, of course, inapplicable to liquids, except water, and almost equally so to light powders, like charcoal.

The figures in column 2, showing the amount of heat produced by a uniform pressure of about 300 atmospheres, or 2 tons, on the square inch, seemed to indicate that in metals the heat produced by compression varied inversely as the atomic weight. The difference in the heat given out by the two kinds of phosphorus is remarkable, and may possibly be held to indicate a wide difference in molecular weight.

The results were not sufficient, either in number or accuracy, to warrant the statement of any such law as that above mentioned, but they were of quite sufficient interest to induce us to take up the subject once more, using a larger number of substances and more accurate methods.

Substance.	Rise of temperature on applying pressure, 300 atmos., deg. cent.	Fall of temperature on suddenly releasing pressure, deg. cent.	Specific heat.	Atomic weight.
Graphite	0·318	0·257	0·188	12
Yellow phosphorus....	0·532	0·408	—	—
,, ,, 2nd	0·955	0·912	0·200	31
Amorphous phosphorus	0·290	0·239	0·170	31
Zinc	0·261	0·221	0·0932	65
Arsenic........	0·261	0·248	0·076	75
Cadmium	0·285	0·293	0·0548	112
Tin	0·277	0·264	0·054	118
Antimony...........	0·248	0·191	0·051	120
Lead	0·305	0·368	0·0315	206
Bismuth	0·251	0·230	0·0305	210
Water..............	0·290	0·201	1·000	—

FIG. 1.—Section of "Gun."



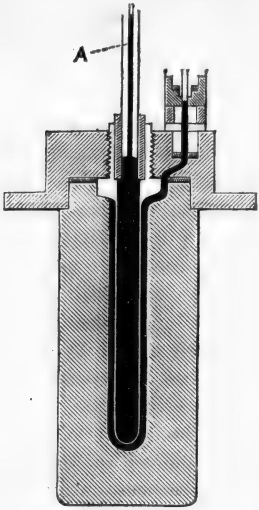
Description of Apparatus and Experimental Methods.

The compression apparatus used in all these various series of experiments was one originally constructed for testing the effect of pressure on thermometers. Its construction is shown in fig. 1. The cylinder, called "the gun," is of wrought iron, about 16 inches long and 4 inches diameter, with a bore of 1 inch. It is fixed vertically, and the lower end is flanged and closed by a very thick flange plate, bolted upon soft leather washers, and drilled to admit water from the pump. The pump-barrel consists of a steel ingot, $1\frac{3}{4}'' \times 6''$, drilled with a hole $\frac{1}{4}$ inch diameter; in this works a plunger, with a steel cup at the lower end, through a stuffing-box and cup leather. The stroke of the pump is about $2\frac{1}{2}$ inches, and it is moved by a handle nearly 3 feet long, with a leverage of about 10 to 1. The upper end of the gun is closed by a solid plunger, turned to fit very accurately, and rendered water-tight by means of a steel cup, turned to a very thin, knife-like edge, slightly belled out, so as to press against the sides of the gun. When pressure comes upon the cup, the sides expand sufficiently to form an almost perfectly tight joint. The plunger is held in position by a key fitting into a hole cut through the sides of the gun and through the plunger or ram. The cup is filled with tallow or lard to avoid leaving an air space inside the gun. The connexion to the pump is formed by a solid-drawn copper tube with a steel connecting piece, which is screwed upon a washer of soft copper.

For measuring the pressure, a compression gauge, designed by Professor Tait, and used by him in his experiments on the "Challenger" thermometers, was employed. It is represented in fig. 2. Essentially it consists of a steel tube, about 5 inches long, $\frac{1}{2}$ inch in diameter, and $\frac{1}{20}$ inch thick, filled with mercury, and bearing a glass capillary, A. This tube is enclosed in a steel vessel communicating with the pump by a solid-drawn copper tube. The pressure is measured by the rise of the mercury in the tube A. The gauge in the first instance was graduated by means of air manometers, and was afterwards compared with an Amagat mercury gauge. Its indications have been found extremely constant. The long bulb contains an inner bulb, which nearly fills up the whole space, leaving only a thin shell of mercury, which is very little affected by temperature. This gauge marked 22·35 mm. per ton pressure. The amount is small, and the reading cannot be said to be very accurate, but no other gauge could be obtained equally trustworthy and which required so very small a bulk of water—a matter of great importance, as will be perceived on referring to the description of the experiments given below.

The wires of the thermo-electric junction are introduced by placing

Fig. 2.—Section of Gauge.



them between two broad washers of thick soft leather, and screwing these as tightly as possible between the flanges at the lower end of the gun. The leathers were prepared by soaking in warm lard *in vacuo*, as recommended by Professor Andrews. The arrangement was found to hold pressure extremely well, if the bolts were tightened up each day before beginning the experiments. As it was necessary that many of the liquids used should be completely protected from water, we were obliged to choose wires for the junction which could be sealed through glass. After experiments with a large number of different pairs of metals, beginning with pure platinum and going up to 33 per cent. of iridium, specimens of commercial platinum and an alloy of platinum with 10 per cent. of iridium were selected. Strange to say, nearly the same current was obtained from a junction of two different samples of 10 per cent. iridium.

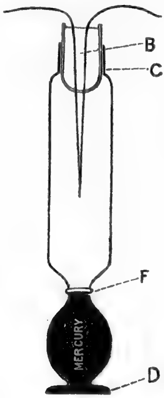
The platinum wire was of about No. 24 B.W.G., the alloy wire of 0″·029 diameter. The latter resembled steel in its properties, being very hard and elastic. Its thickness was a disadvantage, because it increased the mass of the junction and caused a sensible time to elapse before the wires attained the temperature of the liquid during the experiments. The wires were welded to form the junction and hammered thin and flat in order that the contact with the substance under trial might be as perfect as possible. A length of about 3 feet of the double wires was coiled into a spiral spring inside the gun, so that the junction end could be drawn out at the top either for

graduation or to allow the substance to be changed. The wires were insulated by drawing them through fine india-rubber tubing of the kind known as vein tubing. This arrangement proved very objectionable in practice, as the oil and grease off the inside of the gun attacked the rubber and ultimately perforated it, permitting contact with the metal sides of the gun.

Naturally, after this occurred, the results became perfectly irregular and worthless, and the whole apparatus had to be taken to pieces in order to renew the insulation. This was done by stripping off the bad pieces of india-rubber and then covering the whole length of each wire with a double thickness of narrow silk ribbon wound on and whipped over with thread. After the renewal of the insulation there was not a single defective result, and the readings vary within remarkably narrow limits, as will be seen by reference to the tables. The outer or cold junction of the thermo-electric apparatus was immersed in a large wooden tub of cold water, the temperature of which changed but little from day to day, and might be regarded as perfectly uniform during the performance of any experiments.

The apparatus for containing the liquids which were subjected to compression is represented in fig. 3. The wires of the junction are

FIG. 3.—Section of Tube.



fixed into a hollow glass stopper, B, which was ground to fit the tube C. This tube carried a slight flange at D, to which a disc of soft black rubber could be tightly wired. This rubber disc served to close the end of the tube and at the same time allowed perfectly free communication of pressure from the outside to the inside of the apparatus. A loop of thick copper wire was twisted round the neck of the tube to furnish a handle by which it could be drawn out of the gun. In

order to prevent liquids like chloroform or bisulphide of carbon coming in contact with the india-rubber, the tube was filled up to the neck F with clean dry mercury. Twelve tubes were provided, all of which fitted to the same stopper, in order to prevent loss of time in changing substances. The mercury was poured away and a fresh rubber cap wired on for each experiment.

The galvanometer used was of the dead-beat pattern, by White, of Glasgow, of 642 turns and 1·242 ohms resistance. It was placed on a slate shelf about 20 feet distant from the compression apparatus, and it was not at all affected by the movements of the pump handle or by any other part of the mechanism. The scale of the galvanometer was graduated in arbitrary divisions of nearly 2 cm. in length, these being divided into tenths; these small divisions could again be read to quarters. As one large division on the scale corresponded very nearly to 1 degree centigrade, the temperature readings are very accurate. In order to have all substances as nearly as possible at the same temperature, a large covered vessel was sunk in the water-tub which contained also the cold junctions, and in this vessel the bottles of substances were placed the day before, along with mercury and clean tubes ready for use. The suction tube of the pump also drew its supply from the same source.

After the gun had been filled with water taken from the tub, a tube filled with clean mercury to the neck E, and with the substance to be compressed up to C, was then brought up to the stopper and well pressed home, care being taken that no air bubble should be left in inserting the stopper. The wires were arranged on opposite sides of the tube, which was then pressed down on the spiral spring formed by the coiled wire inside the gun, and the ram inserted. The discharge valve at fig. 1 was opened, the ram pressed home and secured by its key. Pressure was usually applied and blown off two or three times to prove that everything was in good condition. As little or no air space was left in any part of the apparatus, the pressure rose very rapidly, two strokes of the pump usually sufficing.

When the galvanometer was steady, its zero point was noted and pressure immediately applied, an operation which took about two seconds. As the apparatus could never be made perfectly tight, it, was always necessary to continue pumping slowly, watching the gauge so as to maintain the pressure as nearly as possible constant till the galvanometer became steady, when the reading was taken and pressure immediately relaxed.

As soon as the galvanometer was again steady a new zero was taken, pressure applied again, and so on, usually ten times. If the results appeared to agree satisfactorily the ram was removed from the gun, and the tube drawn up and carefully examined to see that no leakage had taken place. It was then set aside, a clean tube

taken, and the cycle of operations repeated. In this way three to four substances per hour could be finished, from ten to fifteen observations being taken for each. The limits of accuracy are found in the measurement of pressure, which cannot be said to be quite satisfactory, and in the impossibility of maintaining the pressure perfectly constant till the galvanometer can be read. The error due to the last-named cause is less than might be expected, because the pressure varies rapidly about the point at which it should be maintained, and the galvanometer does not follow the small and quick rises and falls of pressure. In comparing the results among themselves the actual amount of pressure is not of so great importance as the fact that, as nearly as the gauge would read, it was the same in every case, viz., 2·6 tons on the sq. in. = 388 atmospheres.

In order to show the degree of accuracy for this work, the full figures for one substance are here quoted from the experiment book:—

Substance taken, butyric acid. Pressure, 2·6 tons.

Zero point.	Reading.	Deflection.	Rise of temp.
17·0	11·8	5·2	5°·23
17·7	12·5	5·2	5·23
18·1	13·1	5·0	5·03
18·6	13·3	5·3	5·33
18·8	13·7	5·1	5·13
19·0	13·7	5·3	5·33
18·9	13·65	5·25	5·28
19·1	13·9	5·2	5·23
19·1	13·8	5·3	5·33
19·3	14·1	5·2	5 23

Average deflection .... 5·205
,, rise of temperature 5°·235

When the individual figures vary so little among themselves it will easily be seen that the average of ten such observations must differ extremely little from the truth.

At the end of each day's work the junction was graduated to ascertain what deflection of the galvanometer corresponded to a degree centigrade. The graduation was performed in the following manner:—The junction was drawn out of the gun and dipped in a beaker of cold water from the tub to find its zero. As soon as the galvanometer had been read the junction was transferred to a beaker of warm water which was kept rapidly moving by means of a stream of air, and the temperature and the galvanometer were read as nearly as possible at the same moment; the junction was then again placed

in cold water to give a zero and re-transferred to warm water at a different temperature. Care was taken that the greatest temperature difference between the cold and hot water should cause a deflection slightly in excess of the largest caused by compression. The average of four or five such observations is taken as the deflection corresponding to 1° C. The same thermometer was used throughout for all these temperature readings. It was a "fixed zero" by Hicks, graduated in 0°·1 C., and could be read accurately to 0°·01.

To secure the greatest possible uniformity of conditions in all these experiments, every preparation was completed and all substances collected before a single compression was made. The whole of the final observations were thus compressed into a few days, and it was possible to maintain every part of the apparatus practically unchanged during that time.

The Results.

The following table sets forth all the results obtained with liquid specimens, together with such data as may probably be useful in arriving at general conclusions.

Substance.	Formula.	No. of observations.	Rise of temp. in deg. cent.	Maximum variation in deg. cent.
Hydrocarbons—				
Amylene	C_5H_{10}....	11	10·00	0·6
Benzole	C_6H_6	10	6·43	0·8
Alcohols—				
Methyl alcohol	CH_4O ...	12	6·54	0·35
Ethyl ,, ...	C_2H_6O ...	14	4·60	0·8
Propyl ,, ...	C_3H_8O ...	10	6·23	0·35
Isobutyl alcohol.......	$C_4H_{10}O$ ..	20	5·90	0·5
Tertiary butyl alcohol .	$C_4H_{10}O$ ..	—	—	—
Amyl alcohol	$C_5H_{12}O$ ..	15	5·41	0·65
Capryl alcohol........	$C_8H_{18}O$ ..	10	4·28	0·2
Allyl alcohol	C_3H_6O ...	10	4·65	0·2
Aldehydes—				
Aldehyde	C_2H_4O ...	10	8·98	0·75
Paraldehyde..........	$(C_2H_4O)_3$.	11	5·86	0·45
Benzoic aldehyde	C_7H_6O ...	10	5·00	0·2
Acids—				
Formic acid	CH_2O_2 ...	10	3·95	0·1
Acetic ,, ...	$C_2H_4O_2$ ..	10	4·71	0·4
Butyric ,, ...	$C_4H_8O_2$ ..	10	5·19	0·2
Ethereal salts—				
Methyl formate	$C_2H_4O_2$ ..	11	6·29	0·6
Ethyl ,, ...	$C_3H_6O_2$ ..	11	6·52	1·2
Methyl acetate........	$C_3H_6O_2$ ..	10	7·13	0·5

Substance.	Formula.	No. of observations.	Rise of temp. in deg. cent.	Maximum variation in deg. cent.
Ethereal salts (*cont.*)—				
Ethyl acetate	$C_4H_8O_2$ ..	10	7·11	0·4
Propyl „	$C_5H_{10}O_2$..	10	6·58	0·2
Isobutyl „	$C_6H_{12}O_2$..	10	6·65	0·3
Amyl „	$C_7H_{14}O_2$..	10	5·91	0·4
Ethyl oxalate	$C_6H_{10}O_4$..	10	5·31	0·2
Ethyl carbonate	$C_5H_{10}O_3$..	10	5·92	0·3
Acetoacetic ether......	$C_6H_{10}O_3$..	10	5·00	0·3
Ethers—				
Ether	$C_4H_{10}O$ ..	11	7·77	0·3
Amyl ether...........	$C_{10}H_{22}O$..	10	5·69	0·2
Halogen derivatives—				
Chloroform	$CHCl_3$ ...	10	8·19	0·35
Carbon dichloride	CH_2Cl_2...	10	5·85	1·1
Carbon tetrachloride ..	CCl_4.....	10	7·76	0·5
Monochlorethane......	C_2H_5Cl ..	11	8·19	0·7
Acetyl chloride	C_2H_3ClO .	10	7·71	0·3
Dichloracetic acid	$C_2H_2Cl_2O_2$	10	4·17	0·3
Propyl chloride	C_3H_7Cl...	10	8·54	0·7
Isobutyl chloride......	C_4H_9Cl...	12	7·60	0·9
Monochlorbenzole	C_6H_5Cl...	10	6·46	0·45
Ethyl bromide........	C_2H_5Br ..	10	9·09	0·5
Propyl „	C_3H_7Br ..	10	5·49	0·5
Isobutyl „	C_4H_9Br ..	10	5·37	0·65
Amyl „	$C_5H_{11}Br$.	10	5·11	0·35
Monobrombenzole	C_6H_5Br ..	10	5·76	0·4
Bromtoluole	C_7H_7Br ..	10	5·00	0·4
Ethyl iodide..........	C_2H_5I....	11	7·98	0·4
Isobutyl iodide	C_4H_9I....	10	6·64	0·35
Unclassified—				
Acetone	C_3H_6O ...	10	7·36	0·5
Acetic anhydride......	$C_4H_6O_3$ ..	10	5·38	0·3
Carbon disulphide.....	CS_2......	10	8·27	0·4
Inorganic—				
Water	H_2O.....	10	0·303	0·05
Sulphuric acid........	H_2SO_4 ...	10	1·96	0·15
Mercury	Hg......	10	0·829	0·05

Remarks.

Ethyl Alcohol.—Readings not very trustworthy on account of defective insulation.

Tertiary Butyl Alcohol.—Readings exceedingly irregular. On taking out the tube it was found that the substance had solidified. Melted on standing.

Paraldehyde.—On keeping on the pressure after attaining the maximum deflection, a further deflection was observed due to the

crystallising of the substance. The crystals melted rapidly at the temperature of the room.

Monochlorethane.—The whole apparatus was cooled to 0° C. for this series of observations.

Propyl Bromide and Isobutyl Bromide.—A slight "kick" of the galvanometer image in the opposite direction to that indicating a rise of temperature was observed on the first stroke of the pump.

Iodides.—The free iodine was extracted by shaking with mercury.

Carbon Disulphide.—The temperature rose slowly to within a degree of maximum; then there was a sudden rise to maximum, succeeded by a rapid fall.

Looking merely at the rise of temperature produced by pressure, it is impossible to deduce any general laws from these figures. We find that, as a rule, in comparable series, the higher the molecular weight, the less the rise of temperature. This is best seen in the cases of the series of acetates and the halogen substances; but there are several perfectly distinct exceptions—such as the fatty acids—which come in the inverse order to that stated. It is worthy of note that these exceptional series all contain large proportions of oxygen.

We would draw special attention to the effect of pressure on paraldehyde and tertiary butyl alcohol, both of which are caused to solidify at temperatures above their normal melting point. We think it probable that all substances which follow the ordinary law of expansion by heat could be solidified by pressure if tried at temperatures not far from their melting point. The apparatus which we used serves extremely well for observing the course of events in such cases.

When a liquid which does not solidify is compressed, the image on the galvanometer scale travels almost steadily and rapidly to a point at which it remains fixed for a few seconds; it then begins to move back towards zero, as the substance is cooled by the water outside the compression tube. In the two cases mentioned above (paraldehyde and tertiary butyl alcohol) the behaviour of the galvanometer was entirely different, indicating a rapid rise of temperature on application of the pressure, and then a continuous slow increase as long as the pressure was maintained constant. The method of closing the gun by means of a ram renders it easy to remove and inspect a substance within half a minute after letting off the pressure, so that there is not time for the crystals to melt.

Experiments with Solids.

A number of specimens of metals were prepared and tested, with the results given in tabular form below. Of each metal two small

ingots were prepared and filed flat on one face, so that they could be tied one on each side of the flat junction, which was used without any tube. It is difficult in this way to secure at all a satisfactory contact between the sample and junction, and we are of opinion that the method originally used as described in the beginning of the paper will prove more trustworthy. The rise of temperature in metals is so small that, to obtain a readable deflection, it would be necessary to use a more sensitive junction than can be made of platinum-iridium alloys, as the galvanometer cannot be made much more sensitive without becoming too slow to permit of true readings. Considerable care was taken to secure specimens of metals in a state of approximate purity, but, the results having proved of so little value, the methods used need not be here described.

	Rise of temp.
Aluminium	0°·181
Magnesium	0·181
Zinc	0·062
Silver	0·047
Tin	0·125

Several other metals were tried, but the deflection was too slight to allow of accurate observation.

Notwithstanding the large amount of work involved in these experiments, study of the results shows but too clearly that this research can only be regarded as preliminary. Many interesting problems present themselves for solution, of which we think the following are worthy of mention :—

1. The effect of pressure in causing solidification should be followed up and tested with different substances at temperatures little removed from their melting point. In the case of tertiary butyl alcohol it may be noted that, though the crystals formed were too small to be clearly distinguished, they appeared to be of a different habit from those formed under ordinary circumstances.

2. The effect of temperature in causing differences in the amount of heat developed. It might be necessary to compare the substances at different temperatures so as to have them all in the same physical state.

3. The relation of compressibility to heat of compression.

This is but an indication of the few out of the many lines of research suggested by the work we have done. Some of these seem to afford good hope of yielding new knowledge of the constitution of matter.

In conclusion, we wish to record our gratitude to the Government Grant Committee of the Royal Society for affording us the means of

carrying out this research; and also to Professor Tait for allowing us the use of his laboratory and his valuable apparatus, without which these results could not have been obtained.

VIII. "On the Changes evoked in the Circulation and Respiration by Electrical Excitation of the Floor of the 4th Ventricle." By W. G. Spencer, M.S., Assistant-Surgeon to the Westminster Hospital. Communicated by Professor Horsley, F.R.S. Received June 15, 1891.

(Abstract.)

The object of the research was to connect more closely clinical signs with pathological changes in the medulla by localising in the floor of the 4th ventricle the "centres" which influence the circulation and respiration.

The author commences his paper with a full account of the work of previous observers upon the medulla in relation to the circulation and respiration.

The research differs from preceding ones in the use of the electric current to excite without injury the floor of the ventricle, in avoiding puncture and incision of the medulla, in employing complete anæsthesia with ether without at the same time impeding respiration.

The floor of the 4th ventricle was accurately measured in each experiment, so that the distance from the calamus scriptorius and from the middle line of each point was known before it was excited. The experiments were performed on cats, dogs, and monkeys, the records of the changes which took place being divided into those affecting respiration, the rate of the heart, and the blood-pressure respectively.

By adding together the results obtained for each point in all the experiments on animals of the same species, conclusions have been arrived at for each species, and a comparison is then made of the three species of animals.

The conclusions drawn from the experiments, aided by the facts detailed in the historical retrospect, are as follows:—

(1.) *Inspiration.*—The part of the floor of the 4th ventricle which, when excited, caused an increase in the normal inspiratory impulses descending to the thorax lies along the middle line, extending for 2 mm. on either side.

(2.) *Expiration.*—The part of the floor of the 4th ventricle which, when excited, caused an increase in the normal expiratory impulses descending to the thorax lies along the lateral part of the ventricle, 2 to 3 mm. from the middle line.

(3.) *Slowing of the Respiratory Rhythm.*—The area which, when excited, caused slowing of the respiratory rhythm lies over the continuation of the postero-median column, as it separates from the column of the opposite side, and the part of the floor of the ventricle close to the inner border of the column. The central point of this small area lies between 1 and 2 mm. from the calamus, and between 2 and 3 mm. from the middle line.

Cardio-inhibition.—Whilst cardio-inhibition may be produced all over the floor of the 4th ventricle, as well as just behind the calamus, yet it is best marked both in the frequency of occurrence and in the amount of slowing over the posterior third of the 4th ventricle, and over the inner margin of the continuation forwards of the postero-median column.

Blood-pressure. (1) *Fall.*—The chief depressor area is in the hinder part of the floor, between 1 and 4 mm. in front of the calamus.

(2) *Rise* (cat only).—A rise was produced most frequently and most largely from 4 mm. from the calamus forwards to the anterior end of the ventricle.

These conclusions are exhibited graphically by maps of the ventricle, shaded in various degrees to indicate the intensity of the result; and a number of tracings are appended as illustrations of the actual changes which were produced by excitation.

The author concludes with a summary of some clinical applications of this research, as well as of the previous one on "Intracranial Pressure," which forms the subject of a paper in this year's 'Phil. Trans.' by the author and Mr. Horsley.

IX. "Contributions to the Chemistry of Chlorophyll. No. IV." By Edw. Schunck, F.R.S. Received June 16, 1891.

(Abstract.)

This paper is a continuation of the previous ones on the same subject. After describing the action of caustic alkali in a state of fusion on phyllocyanin and the products thereby formed, the author proceeds to give an account of phylloxanthin, the substance formed along with phyllocyanin by the action of acids on chlorophyll. This is followed by a description of the change which chlorophyll undergoes by the action of alkalis, and of the chief product thereby formed, which the author names *alkachlorophyll*.

X. "On some Histological Features and Physiological Properties of the Postœsophageal Nerve Cord of the Crustacea." By W. B. HARDY. Communicated by Dr. GASKELL, F.R.S. Received June 17, 1891.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 19.

Presents, June 18, 1891.

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Eight Photographs of Carboniferous Batrachians. Sir J. W. Dawson, F.R.S.

Appendix to the Report of the Kew Committee for the Year ending December 31, 1890.

Magnetical and Meteorological Observations,

Made at the Kew Observatory, Richmond, Lat. 51° 28′ 6″ N. and Long. $0^h\ 1^m\ 15^s{\cdot}1$ W., height 34 feet above mean sea-level, for the year 1890.

The results given in the following tables are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet K.C. 1. and the Declinometer Magnets marked N.E. and K.O. 90 in the 9-inch Unifilar Magnetometer by Jones.

The Inclination was observed with the Inclinometer by Barrow, No. 33, and needles 1 and 2, which are $3\frac{1}{2}$ inches in length.

The Declination and Force values given in Tables I to VI are prepared in accordance with the suggestions made in the fifth report of the Committee of the British Association on comparing and reducing Magnetic Observations.

The following is a list of the days during the year 1890 which were selected by the Astronomer Royal, as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables.

January	5, 7, 12, 30, 31.
February	2, 7, 10, 23, 25.
March	2, 3, 9, 29, 30.
April..............	3, 9, 18, 25, 28.
May	1, 13, 16, 22, 29.
June	6, 10, 15, 24, 30.
July	3, 9, 14, 28, 29.
August............	4, 12, 13, 28, 30.
September	8, 9, 23, 27, 28.
October............	4, 7, 21, 28, 29.
November..........	3, 6, 11, 24, 29.
December..........	3, 7, 12, 14, 26.

Table I.—Hourly Means of Declination at the Kew Observatory, Richmond, as Month during
(17° + West).

Hours	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
					Winter.						
1890. Months.	′	′	′	′	′	′	′	′	′	′	′
January	52.5	53·1	53·1	52·8	52·6	52·6	52·7	52·3	52·3	52·8	53·6
February....	52·3	52·5	52·9	53·0	52·7	52·4	52·2	51·3	50·8	51·3	52·9
March......	51·7	52·0	51·2	51·1	51·4	50·8	50·4	49·8	49·8	51·5	54·5
October	48·4	48·4	48·1	48·2	48·2	47·8	47·0	46·4	46·0	47·7	50·1
November ..	47·4	47·5	47·7	47·6	47·5	47·2	47·0	46·8	47·1	48·3	50·0
December ..	46·5	47·0	47·0	46·8	46·9	46·7	46·4	46·1	46·2	47·2	48·0
Mean....	49·8	50·1	50·0	49·9	49·9	49·6	49·3	48·8	48·7	49·8	51·5
					Summer.						
	′	′	′	′	′	′	′	′	′	′	′
April	51·4	51·3	51·2	50·7	50·0	49·7	48·5	47·9	48·5	51·0	54·2
May........	50·9	50·5	50·2	49·4	48·4	48·1	47·8	48·1	49·3	52·0	54·6
June........	50·9	50·7	50·1	49·4	48·2	47·2	47·2	47·0	48·1	50·3	52·6
July........	50·8	50·8	50·3	49·5	48·4	47·7	47·4	47·7	49·2	50·7	52·7
August......	49·7	49·5	49·2	48·9	48·1	47·4	47·1	47·2	48·8	51·3	54·4
September ..	48·1	47·9	47·4	47·3	47·2	47·1	46·4	46·6	47·8	49·8	51·5
Mean....	50·3	50·1	49·7	49·2	48·4	47·9	47·4	47·4	48·6	50·9	53·3

Table II.—Solar Diurnal Range of the Kew

Hours..	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
					Summer Mean.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·6	−0·8	−1·2	−1·7	−2·5	−3·0	−3·5	−3·5	−2·3	−0·0	+2·4
					Winter Mean.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·5	−0·2	−0·3	−0·4	−0·4	−0·7	−1·0	−1·5	−1·6	−0·5	+1·2
					Annual Mean.						
	′	′	′	′	′	′	′	′	′	′	′
	−0·6	−0·5	−0·8	−1·0	−1·5	−1·9	−2·2	−2·5	−2·0	−0·3	+1·8

NOTE.—When the sign is + the magne

determined from the Magnetograph Curves on Five selected quiet Days in each the Year 1890.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Winter.												
′	′	′	′	′	′	′	′	′	′	′	′	′
55·3	56·2	55·2	54·4	54·0	53·6	53·2	53·0	52·5	52·2	52·2	52·2	52·3
54·7	55·7	56·0	55·2	54·1	53·3	53·0	52·8	52·2	51·8	51·8	51·4	51·1
56·4	57·3	56·6	54·9	52·7	52·0	51·9	51·7	51·9	51·8	51·8	51·7	51·7
52·1	52·7	52·1	51·1	49·7	49·3	49·0	48·8	48·3	47·1	46·9	47·1	47·6
51·6	51·7	50·2	49·1	48·4	48·0	47·9	47·8	47·5	47·4	47·1	47·3	47·5
48·7	49·0	48·7	47·8	47·1	46·7	46·5	46·5	45·9	45·2	45·4	45·3	45·7
53·1	53·8	53·1	52·1	51·0	50·5	50·3	50·1	49·7	49·3	49·2	49·2	49·3
Summer.												
′	′	′	′	′	′	′	′	′	′	′	′	′
57·0	57·8	56·5	55·0	53·5	52·3	51·9	51·6	51·2	51·7	51·5	51·5	51·1
56·1	56·1	55·5	54·0	52·6	51·6	51·2	50·9	50·9	51·1	51·2	51·3	51·0
54·7	55·6	55·6	55·1	53·9	52·7	52·0	51·4	51·2	51·0	51·1	50·7	50·4
55·2	56·6	56·5	54·9	53·2	51·5	50·8	51·2	51·3	51·4	51·3	51·0	50·5
56·5	56·8	55·4	53·4	51·6	50·4	50·3	50·3	50·4	50·3	50·2	49·9	49·5
53·3	53·7	52·4	50·5	49·7	49·5	49·3	49·3	49·0	49·0	48·7	48·5	48·2
55·5	56·1	55·3	53·8	52·4	51·3	50·9	50·8	50·7	50·8	50·7	50·5	50·1

Declination as derived from Table I.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
Summer Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+4·6	+5·2	+4·4	+2·9	+1·5	+0·4	0·0	−0·1	−0·2	−0·1	−0·2	−0·4	−0·8
Winter Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+2·8	+3·5	+2·8	+1·8	+0·7	+0·2	0·0	−0·2	−0·6	−1·0	−1·1	−1·1	−1·0
Annual Mean.												
′	′	′	′	′	′	′	′	′	′	′	′	′
+3·7	+4·4	+3·6	+2·4	+1·1	+0·3	0·0	−0·2	−0·4	−0·6	−0·7	−0·8	−0·9

points to the west of its mean position.

Table III.—Hourly Means of the Horizontal Force at the Kew Observatory, Temperature) on Five selected quiet
0·18000 + (C.G.S. units).

Hours	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Winter.					
1890. Months.											
January	166	164	166	168	171	172	173	171	167	162	160
February....	168	168	169	171	173	173	174	172	168	162	160
March	173	174	174	173	176	176	175	171	166	158	158
October	168	170	170	171	171	171	168	164	155	150	147
November ..	165	164	166	167	170	170	170	168	164	159	160
December ..	165	165	167	170	172	171	172	171	167	162	161
Mean....	168	168	169	170	172	172	172	170	165	159	158
						Summer.					
April	180	178	179	180	180	178	177	169	159	154	158
May......	187	184	183	181	180	176	173	166	165	165	171
June........	186	185	184	182	181	175	170	166	165	163	164
July........	180	178	180	178	177	175	168	162	158	157	162
August......	175	176	176	175	173	169	163	156	151	152	157
September ..	174	171	172	171	170	166	163	158	150	150	154
Mean....	180	179	179	178	177	173	169	163	158	157	161

(C.G.S. units). Table IV.—Diurnal Range of the Kew

Hours ...	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
						Summer mean.					
	+ ·00004	+ ·00003	+ ·00003	+ ·00002	+ ·00001	− ·00003	− ·00007	− ·00013	− ·00018	− ·00019	− ·00015
						Winter mean.					
	·00000	·00000	+ ·00001	+ ·00002	+ ·00004	+ ·00004	+ ·00004	+ ·00002	− ·00003	− ·00009	− ·00010
						Annual mean.					
	+ ·00002	+ ·00002	+ ·00002	+ ·00002	+ ·00003	+ ·00001	− ·00002	− ·00006	− ·00011	− ·00014	− ·00012

NOTE.—When the sign is + th

Richmond, as determined from the Magnetograph Curves (corrected for
Days in each Month during the Year 1890.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
						Winter.						
159	164	166	167	167	167	167	167	167	166	165	165	166
160	162	167	169	170	172	172	174	174	175	174	173	174
163	168	173	175	173	173	174	176	175	176	176	175	174
151	157	160	165	164	170	171	172	169	169	169	170	169
162	165	169	169	168	171	171	171	169	168	167	168	170
164	168	169	170	168	166	162	163	160	160	160	164	164
160	164	167	169	168	170	170	171	169	169	169	169	170
						Summer.						
166	172	177	181	181	183	182	187	184	185	184	184	184
177	181	185	180	182	186	191	196	193	190	190	193	191
170	177	182	188	184	185	196	195	193	191	186	183	184
171	177	185	189	189	187	187	189	189	188	186	183	180
166	175	179	177	178	176	180	184	182	182	181	180	179
164	168	171	166	168	170	172	171	172	174	171	174	173
169	175	180	180	180	181	185	187	186	185	183	183	182

Horizontal Force as deduced from Table III.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
						Summer mean.						
− ·00007	− ·00001	+ ·00004	+ ·00004	+ ·00004	+ ·00005	+ ·00009	+ ·00011	+ ·00010	+ ·00009	+ ·00007	+ ·00007	+ ·00006
						Winter mean.						
− ·00008	− ·00004	− ·00001	+ ·00001	·00000	+ ·00002	+ ·00002	+ ·00003	+ ·00001	+ ·00001	+ ·00001	+ ·00001	+ ·00002
						Annual mean.						
− ·00008	− ·00002	+ ·00001	+ ·00003	+ ·00002	+ ·00004	+ ·00006	+ ·00007	+ ·00006	+ ·00005	+ ·00004	+ ·00004	+ ·00004

reading is above the mean.

Table V.—Hourly Means of the Vertical Force (corrected for Temperature) at t
the Five selected quiet Days in ea

0·43000 + (C.G.S. units).

Hours	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1890. Months.											
January	972	971	971	971	971	971	972	972	973	970	970
February....	958	957	958	959	960	960	961	962	962	959	956
March	942	943	943	945	947	948	950	951	949	945	941
April	945	946	946	947	949	950	952	950	946	940	935
May	969	969	969	971	973	972	971	965	962	958	954
June	969	970	969	972	973	970	967	964	958	953	951
July	956	956	956	957	958	957	958	956	954	945	940
August......	936	937	937	938	940	941	941	940	935	933	930
September ..	935	936	936	938	938	940	940	940	936	933	931
October	929	929	929	930	930	930	930	931	931	929	925
November ..	—	—	—	—	—	—	—	—	—	—	—
December ...	—	—	—	—	—	—	—	—	—	—	—

NOTE.—During a part of November and December the actic

Table VI.—Hourly Means of the Inclination at the Kew Observato
Five selected qu

67° +

Hours	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
1890. Months.	′	′	′	′	′	′	′	′		′	′
January	33·2	33·3	33·2	33·0	32·8	32·8	32·7	32·9	33·2	33·4	33·5
February....	32·7	32·6	32·6	32·5	32·4	32·4	32·4	32·5	32·8	33·1	33·1
March......	31·9	31·9	31·9	32·0	31·8	31·9	32·0	32·3	32·6	33·0	32·9
April	31·5	31·7	31·6	31·6	31·6	31·8	31·9	32·4	32·9	33·1	32·7
May........	31·7	31·9	32·0	32·2	32·3	32·5	32·7	33·0	33·0	32·9	32·4
June	31·8	31·9	31 9	32·1	32·2	32·5	32·8	33·0	32·9	32·9	32·8
July........	31·8	32·0	31·8	32·0	32·1	32·2	32·7	33·0	33·2	33·1	32·6
August	31·6	31·6	31·6	31·6	31·8	32·1	32·5	33·0	33·2	33·0	32·6
September ..	31·6	31·9	31·8	31·9	32·0	32·3	32·5	32·8	33·3	33·2	32·9
October	31·7	31·8	31·8	31·9	32·0	32·2	32·5	32·9	33·1	33·0	32·7
November...	—	—	—	—	—	—	—	—	—	—	—
December...	—	—	—	—	—	—	—	—	—	—	—

NOTE.—Owing to the doubtful action of the vertical force magnetometer during a part
observed mean values on Nov. 24, 25 and Dec. 22, 24 are inserted in italics.

ew Observatory, Richmond, as determined from the Magnetograph Curves on
Month during the Year 1890.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
970	970	972	973	971	971	970	970	970	969	968	967	967
955	956	960	963	964	964	963	964	964	963	964	963	962
942	942	946	952	956	956	955	957	956	956	956	957	959
934	938	944	949	952	954	954	954	953	952	952	951	952
954	957	961	961	963	965	965	964	962	962	961	962	961
950	954	957	961	966	967	967	968	967	967	967	967	968
941	944	951	955	960	961	959	957	957	955	955	954	954
929	933	941	942	941	941	942	940	940	940	939	939	940
930	932	935	935	935	933	933	933	933	933	934	934	936
924	924	925	926	926	926	925	925	923	923	922	921	921
—	—	—	—	—	—	—	—	—	—	—	—	—
—	—	—	—	—	—	—	—	—	—	—	—	—

the vertical force instrument was not satisfactory.

ulated from the Horizontal and Vertical Forces derived from the
s in each Month.

Noon.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	Mid.
′	′	′	′	′	′	′	′	′	′	′	′	′
33·6	33·3	33·2	33·2	33·1	33·1	33·1	33·1	33·1	33·1	33·1	33·1	33·1
33·1	33·0	32·8	32·7	32·7	32·6	32·5	32·4	32·4	32·4	32·4	32·5	32·4
32·6	32·2	32·0	32·0	32·3	32·3	32·2	32·1	32·2	32·1	32·1	32·2	32·3
32·1	31·9	31·7	31·6	31·6	31·6	31·6	31·3	31·5	31·4	31·4	31·4	31·4
32·0	31·8	31·6	32·0	31·9	31·7	31·3	31·0	31·1	31·3	31·3	31·1	31·2
32·3	32·0	31·7	31·4	31·8	31·8	31·1	31·2	31·3	31·4	31·7	31·9	31·9
32·0	31·7	31·5	31·2	31·3	31·5	31·4	31·3	31·3	31·3	31·4	31·6	31·8
32·0	31·5	31·5	31·6	31·5	31·7	31·4	31·1	31·2	31·2	31·3	31·3	31·4
32·2	32·0	31·8	32·2	32·0	31·8	31·7	31·8	31·7	31·6	31·8	31·6	31·7
32·1	31·8	31·6	31·7	31·7	31·7	31·4	31·3	31·3	31·4	31·5	31·5	31·6
—	—	—	*32·2*	—	—	—	—	—	—	—	—	—
—	—	—	*31·7*	—	—	—	—	—	—	—	—	—

vember and December, the inclination has not been calculated for those months, but the

APPENDIX II.—Table I.

Mean Monthly Results of Temperature and Pressure for Kew Observatory.
October, 1889, *to December*, 1890.

Months.	Thermometer.								Barometer.*					Mean vapour-tension.
	Mean.	Means of—			Absolute Extremes.				Mean.	Absolute Extremes.				
		Max.	Min.	Max. and Min.	Max.	Date.	Min.	Date.		Max.	Date.	Min.	Date.	
1889.	°	°	°	°	°	d. h.	°	d. h.	ins.	ins.	d. h.	ins.	d. h.	in.
Oct.....	48·2	54·3	42·4	48·4	59·1	16 1 P.M.	32·0	13 6 A.M.	29·702	30·231	25 11 P.M.	29·129	19 10 A.M.	·300
Nov. ...	44·4	49·3	39·1	44·2	57·1	15 2 ,,	29·3	30 Midt.	30·235	30·687	18 9 ,,	29·310	25 4 ,,	·260
Dec. ...	37·7	41·7	32·7	37·2	52·0	17 1 ,,	22·7	29 6 A.M.	30·208	30·668	5 {8 ,, / 11 ,,}	29·295	10 5 P.M.	·204
1890.														
Jan.....	43·8	48·3	38·6	43·5	54·1	25 2 P.M.	21·9	1 5 P.M.	29·948	30·480	29 11 P.M.	28·682	23 Noon	·246
Feb. ...	37·8	42·7	33·7	38·2	49·8	17 3 ,,	27·5	11 9 A.M.	30·209	30·720	23 11 A.M.	29·412	15 3 P.M.	·190
March..	43·3	49·7	36·4	43·1	64·2	28 2 ,,	18·3	4 7 ,,	29·854	30·513	3 11 ,,	29·164	16 5 ,,	·223
April...	45·5	53·0	38·7	45·9	62·7	30 1 ,,	30·8	5 6 ,,	29·836	30·395	1 9 ,,	29·271	25 9 A.M.	·229
May ...	54·1	62·8	44·7	53·8	73·4	24 2 ,,	37·9	3 4 ,,	29·846	30·331	22 8 ,,	29·377	11 5 ,,	·300
June ...	57·9	66·3	50·4	58·4	75·7	25 4 ,,	38·3	1 4 ,,	30·013	30·418	15 9 ,,	29·183	30 7 P.M.	·371
July ...	59·4	66·8	52·5	59·7	74·3	23 5 ,,	44·0	12 4 ,,	29·916	30·265	20 11 ,,	29·206	1 3 A.M.	·389
Aug....	59·0	66·8	52·0	59·4	77·3	5 5 ,,	40·5	31 5 ,,	29·900	30·324	31 Midt.	29·389	27 1 P.M.	·383
Sept....	58·8	68·0	50·5	59·3	73·1	16 2 ,,	36·9	1 5 ,,	30·161	30·503	25 11 P.M.	29·597	21 {9 ,, / 11 ,,}	·394
Oct.....	48·9	56·8	41·7	49·3	66·2	4 2 ,,	25·5	28 7 ,,	30·116	30·552	22 10 ,,	29·406	26 1 ,,	·273
Nov. ...	43·4	48·6	37·0	42·8	58·3	23 5 ,,	21·5	{28 4 P.M. / 30 9 A.M.}	29·885	30·512	20 10 A.M.	29·003	7 3 A.M.	·236
Dec. ...	30·0	33·4	25·3	29·4	43·8	4 Noon	10·8	22 8 P.M.	30·048	30·402	24 10 P.M.	29·232	19 7 ,,	·128
Yearly Means	48·5	55·3	41·8	48·6	..	....	..	....	29·978	..	....	..	....	·280

This Table is compiled from "Hourly Means," vols. 1889 and 1890, of the Meteorological Office.
* Reduced to 32° at M.S.L.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=overcast).	Rainfall.* Total.	Rainfall.* Maximum.	Rainfall.* Date.	Weather. Number of days on which were registered: Rain. ‡	Snow.	Hail.	Thunderstorms.	Clear sky.	Overcast sky.	Gales.	Wind.† Number of days on which it was: N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Calm.
1889.		in.	in.																	
October	7·2	3·990	0·675	19	23	..	..	1	2	13	..	6	3	1	2	7	9	2	1	9
November ...	7·5	0·720	0·260	24	7	2	..	..	4	17	..	2	1	5	4	3	6	5	4	13
December ...	7·4	1·200	0·275	21	18	2	..	..	4	18	..	2	5	3	2	3	10	5	1	11
1890.																				
January	7·4	2·170	0·360	27	22	..	1	..	3	15	4	1	..	1	1	7	12	6	3	2
February	6·7	0·900	0·625	14	7	3	..	..	5	11	..	7	7	8	1	1	1	2	1	3
March	6·7	1·530	0·370	19	16	3	2	..	3	11	..	2	2	1	1	4	11	7	3	..
April	6·1	1·735	0·305	25	15	..	..	..	6	10	1	7	6	6	..	2	4	3	2	3
May	5·6	1·415	0·520	9	12	..	..	..	5	5	..	4	4	5	2	8	3	2	3	2
June	7·5	3·385	0·960	28	16	..	..	2	..	15	..	2	1	..	1	4	12	8	2	4
July	7·5	4·455	2·285	17	14	..	..	2	..	17	..	4	1	1	..	2	11	8	4	1
August	6·2	1·950	0·710	19	15	..	..	1	2	9	..	4	5	1	..	1	12	6	2	6
September ...	5·2	0·585	0·220	17	7	..	1	1	8	9	..	1	1	2	5	5	8	5	3	7
October	5·3	1·025	0·295	25	16	..	..	..	10	9	..	1	1	..	..	3	12	7	7	11
November ...	7·6	1·525	0·375	6	18	2	..	..	1	15	1	2	3	1	1	6	7	8	2	3
December ...	9·4	0·545	0·185	18	9	10	..	..	..	26	1	2	15	10	2	1	1	..	..	12
Totals and mean for 1890	6·8	21·220			167	18	4	6	43	152	7	37	46	36	14	44	94	62	32	54

* Measured at 10 A.M. daily by gauge 1·75 feet above ground. † As registered by the anemograph.

‡ The number of rainy days are those on which 0·01 rain or melted snow were recorded.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine. Total number of hours recorded.	Bright Sunshine. Mean percentage of possible sunshine.	Bright Sunshine. Greatest daily record.	Bright Sunshine. Date.	Maximum temperature in sun's rays. (Black bulb *in vacuo.*) Mean.	Maximum temperature in sun's rays. Highest.	Maximum temperature in sun's rays. Date.	Minimum temperature on the ground. Mean.	Minimum temperature on the ground. Lowest.	Minimum temperature on the ground. Date. †	Horizontal movement of the air.* Average hourly velocity.	Horizontal movement of the air.* Greatest hourly velocity.	Horizontal movement of the air.* Date.
1889.	h. m.		h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
October	83 18	25	7 36	12, 31	93	112	10	37	30	10	8	31	7
November	42 6	26	7 30	2	71	104	1	33	19	27	7	30	24, 25
December	31 12	13	5 6	25	56	78	23	27	15	4	8	31	20
1890.													
January	56 0	21	6 30	12, 29	74	94	27	34	17	1	14	42	25
February	57 48	21	6 36	3	72	98	16	29	21	12	12	31	19
March	109 18	30	11 12	30	97	115	26	30	11	4	12	30	8
April	144 48	35	12 54	29	102	125	30	32	20	2	12	39	14
May	223 54	46	13 48	21, 23, 24	116	132	21	39	30	3	10	31	20, 24
June	141 24	29	12 18	7	125	139	9	45	29	1	9	23	3
July	139 54	28	12 18	16	123	138	24	49	38	12	9	27	5
August	182 30	41	11 42	17	122	140	4	49	33	31	9	29	16
September	169 30	45	10 30	16	117	130	5	45	31	1	7	28	20
October	121 36	39	10 12	3	94	117	4	36	16	28	8	27	16
November	57 36	21	6 18	9	75	95	1, 4	31	15	30	10	36	7
December	0 18	0·1	0 12	7	38	61	7	21	7	23	9	35	5
Total and Means for 1890	1404 36	30	..	..	96	..	..	37	..	..	10	..	..

* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

† Read at 10 A.M., and entered to same day.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1889.			
October	17	1	13
November	11	0	11
December	9	3	5
1890.			
January	14	2	7
February	14	0	14
March	18	1	14
April	18	1	16
May	22	5	10
June	18	1	14
July	19	3	8
August	17	3	8
September	21	6	3
October	17	2	11
November	15	2	7
December	1*	*	*
Totals for 1890	194	26	112

* The Sun was only faintly visible on two days during the month.

Table IV.

Summary of Sun-spot Observations made at the Kew Observatory.

Months.	Days of observation.	Number of new groups enumerated.	Days apparently without spots.
1889.			
October................	17	1	13
November.............	11	0	11
December	9	3	5
1890.			
January	14	2	7
February..............	14	0	14
March	18	1	14
April..................	18	1	16
May..................	22	5	10
June	18	1	14
July..................	19	3	8
August	17	3	8
September............	21	6	3
October..............	17	2	11
November............	15	2	7
December	1*	*	*
Totals for 1890	194	26	112

* The Sun was only faintly visible on two days during the month.

November 19, 1891.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

Mr. William Anderson and Professor Frederick Orpen Bower were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Sir James Cockle, Mr. F. Galton, and Mr. Stainton were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers were read:—

I. "The Thermal Emissivity of Thin Wires in Air." By W. E. AYRTON, F.R.S., and H. KILGOUR. Received July 2, 1891.

(Abstract.)

In 1884 it was observed experimentally that whereas the electric current required to maintain a *thick* wire of given material, under given conditions, at a given temperature was approximately proportional to the diameter of the wire raised to the power three halves, the current was more nearly proportional to the first power of the diameter if the wire were *thin*. When this difference in the behaviour of a thick and thin wire was first noticed it was regarded as being quite unexpected. But, as pointed out by one of us in the course of a discussion at a meeting of the Royal Society, the unexpected character of the result was due to people having assumed that the loss of heat from radiation and convection per square centimetre of surface per 1° excess temperature was a constant, and independent of the size and shape of the cooling body.

The very valuable investigations that have been made on emissivity by Mr. Macfarlane, Professor Tait, Mr. Crookes, Mr. J. T. Bottomley, and by Mr. Schleiermacher had for their object the determination of the variation of the emissivity with changes of the surface and with change in the density of the gas surrounding the cooling body, but it was not part of these investigations to determine the change in the

emissivity that is produced by change in the shape and size of the cooling body. Indeed, so little has been the attention devoted to the very large change that can be brought about in the value of the emissivity by simply changing the dimensions of the cooling body, that in Professor Everett's very valuable book of Units and Physical Constants the absolute results obtained by Mr. Macfarlane are given as the "results of experiments on the loss of heat from blackened and polished copper in air at atmospheric pressure," and no reference is made either to the shape or to the size of the cooling body.

[November 19, 1891.—Since this paper was sent in to the Royal Society, a new edition of this book has appeared, and, in consequence of a suggestion made to Professor Everett, the word "balls" has been added after the word "copper" in this new edition, as well as the following paragraph:—

"*Influence of Size.*

"According to Professor Ayrton, who quotes a table in 'Box on Heat,' the coefficient of emission increases as the size of the emitting body diminishes, and for a blackened sphere of radius r cm. may be stated as

$$\text{“}\, 0\cdot0004928+\frac{0\cdot0003609}{r}.$$

"The value in M'Farlane's experiments was 2."]

The laws which govern the loss of heat from thin cylindrical conductors have not only considerable scientific interest in showing how the shape of a body affects the convection currents, but they are of especial importance to the electrical engineer in connexion with glow lamps, hot-wire voltmeters, fuses, &c. We, therefore, thought it desirable to ascertain the way in which the law of cooling for thick wires, which involved the diameter raised to the power three halves, passed into the law for the cooling of thin wires, involving only the first power of the diameter. For this object, the investigation described in the paper was commenced at the beginning of 1888, and the emissivity was measured of nine platinum wires, having the diameters of 1·2, 2·0, 2·9, 4·0, 6·0, 8·1, 9·3, 11·1, and 14 mils, or thousandths of an inch.

Suspecting that some of the published results concerning the currents required to fuse wires had been much influenced by the cooling action of the blocks to which the ends of the wires were attached, we started by making a calculation of the length necessary to give to our wires, so that the loss of heat by conduction should not introduce any important error into the determination of the emissivity. To do this it was necessary to calculate the distribution of temperature along a

wire through which a steady current was flowing and from which heat was lost by radiation, convection, and conduction, and it was further necessary to improve on the calculation one of us had published on this subject in the 'Electrician' for 1879, by taking into account the fact that the emissivity, as well as the thermal and electric conducting power, of the wire differed at different points in consequence of the difference of temperature.

Until we had completed the experiments described in this paper we could, of course, only employ in this calculation values that we had guessed at as being something near the truth for the emissivity of platinum wire for different diameters and at different temperatures. Hence, after the completion of the experiments, we took up the mathematical investigation again, substituting for the emissivity such a function of the diameter of the wire and the temperature of the point as we had experimentally found it to be. Section IV of the paper contains the investigation by which we finally arrived at the calculated distribution of temperature along the wire, and we have to express our sincere thanks to Professor Henrici (whom we consulted as to the best method of practically solving the rather complex differential equation arrived at) for the warm interest that he has taken in the mathematical treatment of the subject, and for the many suggestions which he has made, and which have enabled us to arrive at the mathematical solution given in the paper.

Each wire to be tested was stretched along the axis of a water jacketed cylinder 32·5 cm. long, the inner surface of which was blackened and kept at a constant temperature by a stream of water flowing through the jacket. The rate at which heat was lost by any one of the wires was measured by the product of the current passing through it into the P.D. (potential difference) maintained between its ends, while the ratio of the P.D. to the current gave the resistance of the wire and, therefore, its temperature. Experiments were in this way made with various currents flowing through each of the nine wires.

As the variation of resistance with temperature is known to vary with different specimens of platinum, experiments were separately made to determine the actual law of variation of resistance with temperature up to 300° C. for each piece of wire that had been employed in the emissivity experiments.

In this later determination various thermometers were used, and the subsequent comparison of these thermometers with a Kew standard thermometer involved a vast amount of labour, from the fact that it is, or at any rate was not possible three years ago, to purchase from the Kew Observatory a standard thermometer reading from, say, 200° to 300° C., with a short, wide chamber at the base in which the mercury expanded below 200° C. All that could be obtained was a

long thermometer which had been carefully tested between 0° and 100° C., and the remainder of whose tube had been simply calibrated for uniformity of bore. The consequence was that when we desired to compare one of our thermometers reading, say, from 200° to 300° C., with the Kew standard, their bulbs were very far apart when both were immersed in the oil-bath, and with the tops of the mercury columns just above the surface of the oil. A short description is given in the paper of the devices employed to overcome this difficulty and which enable an accurate comparison to be made between the thermometers.

On examining the curves accompanying the complete paper which show the emissivity for each temperature for each of the nine wires, we see that:—

1. For any given temperature the emissivity is the higher the finer the wire.

2. For each wire the emissivity increases with the temperature, and the rate of increase is the greater the finer the wire. For the finest wire the rate of increase of emissivity with temperature is very striking.

3. Hence the effect of surface on the total loss of heat (by radiation and convection) per second per square centimetre per 1° C. excess temperature increases as the temperature rises.

On comparing the loss of heat from the wire of 1·2 mils diameter when at 300° C. with that from the wire of 6 mils diameter when at 15° C., both being in an enclosure at 10° C., we see that the former loses per square centimetre of surface per second not

$$\frac{300-10}{15-10}, \text{ or } 58 \text{ times}$$

as much heat as the latter, as it would if the emissivity were the same; but, instead,

$$60 \times 58 \text{ or } 3480 \text{ times}$$

as much heat; arising from the fact that the emissivity, that is, the number of calories (gramme C.°) lost per second per square centimetre of surface per 1° C. excess temperature of the 1·2-mil wire at 300° C., is 60 times as great as that of the 6-mil wire at 15°, the latter varying very rapidly with the temperature near 15° C.

From the curves the following table (p. 170) has been drawn up, giving the emissivities of the various wires at eight useful temperatures.

We find that the emissivity of platinum wires of different diameters at the same temperature can be very fairly expressed by a constant *plus* a constant into the reciprocal of the diameter of the wire. For example, we find that

Diameter of wire in		Emissivities.							
Mils.	Millimetres.	40° C.	60° C.	80° C.	100° C.	150° C.	200° C.	250° C.	300° C.
1·2	0·0305	0·008230	0·009560	0·010300	0·010846	0·011875	0·012783	0·013625	0·014400
2·0	0·0508	0·005950	0·006860	0·007500	0·007900	0·008600	0·009070	0·009480	0·009850
2·9	0·0737	0·002193	0·003336	0·004086	0·004552	0·005095	0·005379	0·005628	0·005845
6·0	0·1524	0·002460	0·002660	0·002806	0·002930	0·003212	0·003460	0·003666	0·003837
8·1	0·2057	—	—	—	0·002804	0·002939	0·003076	0·003217	0·003352
9·3	0·2362	—	—	—	0·002297	0·002448	0·002586	0·002718	0·002843
11·1	0·2819	—	—	—	0·002053	0·002216	0·002363	0·002490	0·002608
14·0	0·3556	—	—	—	0·001894	0·002027	0·002136	0·002224	0·002286

The wire of 4 mils diameter is omitted from the table, as the experiments showed that its specific resistance was much greater, its temperature coefficient much smaller, and its emissivity much smaller than if it had been of platinum. This piece of wire probably therefore contained iridium or silver.

At 100° C. $e = 0{\cdot}0010360 + 0{\cdot}0120776d^{-1}$ .... (1),
„ 200 „ $e = 0{\cdot}0011113 + 0{\cdot}0143028d^{-1}$ .... (2),
„ 300 „ $e = 0{\cdot}0011353 + 0{\cdot}016084\,d^{-1}$ .... (3),

where d is the diameter of the wire in mils, or thousandths of an inch.

The emissivities have been calculated in calories lost per second per square *centimetre* per 1° C. excess temperature, in order that they may be compared with the emissivities obtained by other experimenters, but we prefer to speak of the diameters of the wires in *mils*, since a wire of 1 mil is about the finest that is drawn commercially. Hence the statement that the diameters of wires are 1, 2, or 3 mils is more suggestive to an engineer than saying that they are 0·0254, 0·0508, or 0·0762 millimetres.

The statement, not unfrequently made, that the current required to maintain a wire of a given material at a given temperature above that of the surrounding envelope is proportional to the diameter of the wire raised to the power three halves, is equivalent to stating that the emissivity is independent of the diameter. Now from the three formulæ (1), (2), (3), given above for e, we may conclude—

That for a temperature of 100° C. the value of d in the formula

$$e = 0{\cdot}0010360 + 0{\cdot}0120776d^{-1}$$

must be something like 220 mils, or 5·6 mm., in order that the neglect of the second term may not make an error in e of more than 5 per cent., and something like 1·15 inch, or 29·3 mm., if the error is not to exceed 1 per cent.;

That for a temperature of 200° C. the value of d in the formula

$$e = 0{\cdot}0011113 + 0{\cdot}0143028d^{-1}$$

must be something like 244 mils, or 6·2 mm., in order that the neglect of the second term may not make an error in e of more than 5 per cent., and something like 1·28 inches, or 32·5 mm. if the error is not to exceed 1 per cent.;

And that for a temperature of 300° C. the value of d in the formula

$$e = 0{\cdot}0011353 + 0{\cdot}016084d^{-1}$$

must be something like 267 mils, or 6·8 mm., in order that the neglect of the second term may not make an error in e of more than 5 per cent., and something like 1·39 inches, or 35·3 mm., if the error is not to exceed 1 per cent.

Generally, then, we may conclude that to assume that the emissivity is a constant for wires whose diameters vary from a small value up to 1 inch is to make a large error in the case of the greater number of

the wires, and an error of hundreds per cent. in the case of some of them.

Using the formula (3) which we have arrived at for determining the emissivity of platinum wires of different diameters at 300° C., it follows that to maintain a platinum wire 0·75 mil in diameter at 300° C. would require a current density of 331,000 amperes per square inch, and, if the emissivity of a copper wire of the same diameter and at the same temperature may be taken as being the same, it follows that to maintain a copper wire 0·75 mil in diameter at 300° C. would require a current density of 790,000 amperes per square inch.

II. "On the Time-Relations of the Excursions of the Capillary Electrometer, with a Description of the Method of using it for the Investigation of Electrical Changes of Short Duration." By GEORGE J. BURCH, B.A. Oxon. Communicated by Professor BARTHOLOMEW PRICE, F.R.S. Received September 3, 1891.

(Abstract.)

This paper is in continuation of the author's preliminary note "On a Method of determining the Value of Rapid Variations of a Difference of Potential by means of the Capillary Electrometer," and describes a further simplification of the method then brought forward, consequent on a change in the mode of producing the photographic record of an excursion.

The rapidity of the movement of the meniscus was found to be affected by (1) the degree of concentration of the acid, (2) the length of the capillary beyond the end of the mercury column, (3) the shape of the tube where it tapers to form the capillary, (4) the shape of the orifice. These things might be taken as indicating the action of both mechanical friction and electrical resistance in determining the rate of movement. As was announced in the preliminary note, under ordinary circumstances the instrument is perfectly dead-beat; the meniscus commences to move the instant a difference of potential is communicated to the instrument, and stops directly it is withdrawn. The conditions under which overshooting may occur, and the possible extent of it, are discussed. It was found that, in general, the time-relations of the movement might be expressed by the equation

$$y = ae^{-ct},$$

in which y is the distance of any point upon the curve from its asymptote. The tabular logarithms of a series of ordinates corre

sponding to equal time-intervals are in arithmetical progression, and the sub-tangent to the curve is of constant length. It was also shown in the preliminary note that the tangent to a point on the curve produced by an irregular change of electromotive force coincides in direction with that of the normal curve produced by the difference of potential existing at that instant between the terminals of the electrometer, and on this was based a method of determining the amount of that difference.

Further Investigation of the Formula $y = ae^{-ct}$.

Calibration Error.—The greater the range of the excursion for a given small difference of potential, the slower is the action of the instrument. Hence, in the majority of capillaries, the rate of movement *decreases* as the meniscus approaches the tip of the capillary.

Change of Resistance.—The shorter the length of dilute acid, the smaller is the resistance, and the quicker is the motion; hence, in all instruments there is a tendency to *increased rapidity* as the meniscus approaches the tip of the capillary. The equivalent internal resistance of an electrometer may be written

$$R = r(L+l),$$

where l = the length of the capillary beyond the meniscus at any moment, and L = a constant many times larger than l, and representing the sum total of the mechanical and the remaining electrical resistances. The effect of the change of resistance is so much smaller that it may be completely masked and neutralised by the calibration error, which has an opposite effect.

Change in the Mode of Photographing the Excursions.

In order to bring out the details of the electrical phenomena of muscle, it was necessary to make the plates move faster than was possible with the apparatus hitherto employed. To do this they were attached to a kind of balanced pendulum and caused to describe an arc of a circle. With this arrangement the normal excursion is best expressed in polar coordinates. Time being recorded on a circular arc, t becomes θ. Instead of the rectilinear asymptote there is an asymptotic circle of radius = R. The expression for the radius vector is $r = R \pm y$, the equation connecting y and θ being $y = ae^{-c\theta}$. With such a curve the method of analysis first put forward is no longer applicable. In place of it however there is a still simpler one. The equation to the polar sub-normal, $r \cot \psi = dr/d\theta$, is in this case independent of R, being simply cy. In other words, the polar subnormal to a point on the curve is a constant multiple of its distance

from the asymptotic circle, and consequently, by the same reasoning on which the previous method of analysis was based, represents, on a scale which can be easily determined, and which is constant so long as the resistance in circuit is unchanged, the difference of potential between the terminals of the electrometer, *minus* or *plus* the difference of potential indicated by the *position* of the meniscus above or below the zero-line.

Analysis of a Normal Curve.

Full details are given of the measurement of a normal curve, the equation to which was shown to be $y = ae^{-c\theta}$, the maximum difference between the observed and the calculated values being only 0·071 mm. The constant multiplier in this case was $c = 8{\cdot}50$. Details of five other smaller excursions of known value are also given, showing that the error in determining differences of potential by this method is less than 1 per cent.

Artificial Spikes.

This name was given to excursions produced by two currents in opposite directions, each lasting about 0·005 second. It was intended by this means to investigate the effects of overshooting, and also to ascertain whether the electrometer was capable of discriminating between a current of definite strength suddenly communicated to it, and a more or less gradual rise of a difference of potential extending over a period of equal duration. That it can do so was clearly established. The effect of the elasticity of the meniscus, and of overshooting proper, is shown. With no resistance in circuit it did not exceed 0·01 of the full excursion, and was rendered inappreciable by the introduction of a few thousand ohms.

Part II.

Application of the Method to the Study of the Electrical Variations of Muscle.

After a brief sketch of the problem under consideration and the mode in which the physiological experiments were made, the author describes minutely the manner in which a muscle-curve is analysed. In order to illustrate the kind of information which can be thus obtained, he gives a series of specimen records of the electrical variations of the gastrocnemius of the frog, together with the analysis of each. The interpretation of the results, from a physiological standpoint, he desires to leave entirely in the hands of Professor Burdon Sanderson, to whom he is indebted for permission to make use of the photographs.

III. "On the Collision of Elastic Bodies." By S. H. Burbury, F.R.S. Received October 24, 1891.

(Abstract.)

1. In this paper I discuss, firstly, a case suggested by Sir W. Thomson on June 11 as a test of the truth of the Maxwell-Boltzmann doctrine concerning the distribution of energy. Sir W. Thomson supposes a number of hollow elastic spheres, each containing a smaller sphere free to move within it. This pair he calls a doublet.

If V be the velocity of the centre of inertia of a doublet, R the relative velocity of the two spheres, then, under the distribution in question, for given direction of R, all directions of V are equally probable, and the converse is also true. If a collision occurs, the change of direction of R due to it is independent of the direction of V, as well as of the magnitudes of V and R. Therefore, after collisions, as well as before, for given direction of R all directions of V are equally probable. Whence it follows that the distribution of velocities is unaffected by collisions. This appears to me to be sound as well for internal as for external collisions.

2. The characteristic of collisions of conventional elastic bodies is the discontinuous change in the velocities without alteration of the kinetic energy. If that occurs for any material system of n degrees of freedom, there are $n-1$ independent linear functions of the velocities $v_1 \ldots . v_n$ which remain unaltered, call them $S_1 \ldots . S_{n-1}$, and one, R, which is unaltered in magnitude but reversed in sign.

3. The kinetic energy cannot contain any of the products RS, but must be of the form $2E = \lambda R^2 + f(S_1 \ldots . S_{n-1})$, where $f(S_1 \ldots . S_{n-1})$ is a quadratic function of these quantities.

4. If after collision the velocities $v'_1 \ldots . v'_n$ were all reversed in sign, R and $S_1 \ldots . S_{n-1}$ would be reversed in sign. The system would retrace its course, undergoing collision, changing v' into v. R would be positive before and negative after collision. $S_1 \ldots . S_{n-1}$ would be throughout negative, *i.e.*, of opposite signs to the signs they had in the first case.

5. To define a collision, we assume that a certain function ψ of the coordinates and constants cannot become positive, and when $\psi = 0$, $d\psi/dt$ being positive, $d\psi/dt$ changes sign discontinuously, and a collision occurs. It follows that $d\psi/dt$ is equal or proportional to R.

6. What has been proved for a system holds equally for a pair of systems, having coordinates $p_1 \ldots . p_r$ for the one, and $p_{r+1} \ldots . p_n$ for the other, if ψ be a function of $p_1 \ldots . p_n$, which cannot become positive.

7. All those systems for which, at a given instant, ψ lies between

zero and $-(d\psi/dt)\delta t$, $d\psi/dt$ being positive, will undergo collision within the time δt after that instant. Therefore $d\psi/dt$ or R measures the frequency of collision.

8. From the linear equations connecting $v_1 \ldots . v_n$ with $S_1 \ldots . S_{n-1}$ and R, we can find, say, v_1 as a linear function of $S_1 \ldots . S_{n-1}$ R, and v'_1 as the same function of $S_1 \ldots . S_{n-1}$ and $-R$. Therefore $v_1^2 - v'^2_1 = 4R\Sigma\,\mu S$, and $(v_1^2 - v'^2_1)R = 4R^2\Sigma\,\mu S$, where the μ's are functions of the coordinates and constants. Now let $S_1 \ldots . S_{n-1}$ go through all values consistent with

$$2E = \lambda R^2 + f(S_1 \ldots . S_{n-1}),$$

and let $\phi(S_1 \ldots . S_{n-1})dS_1 \ldots . dS_{n-1}$ be the number in unit volume of systems for which they lie between

$$S_1 \text{ and } S_1 + dS, \text{ \&c.},$$

given E and R and the coordinates within certain limits.

Then

$$\iiint \ldots . (v_1^2 - v'^2_1)\, R\phi\,(S_1 - S_{n-1})\, dS_1 \ldots . dS_{n-1}$$
$$= 4R^2 \iiint \ldots . \phi\,(S_1 - S_{n-1})\, \Sigma\mu S\, dS_1 \ldots . dS_{n-1}.$$

Now, in the Maxwell-Boltzmann distribution, $\phi(S_1 \ldots . S_{n-1})$ is a function of the kinetic energy only, and, therefore, constant throughout this integration. Therefore

$$\iiint \ldots . (v_1^2 - v'^2_1)\, R\, dS_1 \ldots . dS_{n-1} = 4R^2 \iint \ldots . \Sigma\,\mu S\, dS_1 \ldots . dS_{n-1}$$
$$= 0,$$

because for every set of values of $S_1 \ldots . S_{n-1}$ there is included in the integration another set with reversed signs.

Now $\iiint \ldots . (v_1^2 - v'^2_1)/R\, dS_1 \ldots . dS_{n-1}$ expresses the mean value of $v_1^2 - v'^2_1$ for all collisions, given E and R, and since it is zero, the distribution of velocities is not altered by collision, or the Maxwell-Boltzmann distribution, given existing, is not affected by collisions.

9. Certain examples are given showing the values of $S_1 \ldots . S_{n-1}$ and R in given cases, viz.:—I. Elastic spheres of masses M and m. II. Elastic spheres colliding with spheroids.

10. Professor Burnside's problem of a set of equal and similar spheres, each of which, instead of being homogeneous, has its centre of inertia at a small distance c from the centre of figure. Discussion of his result, which does not agree with the Maxwell-Boltzmann doctrine, owing, as I believe, to an oversight.

11. A general proof is now given of the permanence of the distribution, viz., if there be a set of systems called system M, each having

coordinates $p_1 \ldots . p_r$, and another set called systems m with coordinates $p_{r+1} \ldots . p_n$. Let $\mathrm{F}(p_1 \ldots . p_r\, v_1 \ldots . v_r) dp_1 \ldots . dv_r$ or $\mathrm{F} . dp_1 \ldots . dv_r$ be the number of systems M with coordinates and velocities between

$$\left.\begin{array}{c} p_1 \text{ and } p_1 + dp, \\ \&c., \\ v_1 \text{ and } v_1 + dv_1, \\ \&c., \end{array}\right\} \ldots\ldots\ldots\ldots \text{(A)},$$

and $f(p_{r+1} \ldots . p_n\, v_{r+1} \ldots . v_n) dp_{r+1} \ldots . dv_n$ the corresponding number for the other set, between limits

$$p_{r+1} \text{ and } p_{r+1} + dp_{r+1}, \&c. \ldots\ldots\ldots\ldots \text{(B)}.$$

Let ψ be a function, such that when $\psi = 0$ a collision occurs. Then $d\psi/dt$ or R denotes the frequency of collision. And $\mathrm{F}f . \mathrm{R} . dp_1 \ldots . dv_{n-1}$ denotes the number in unit of time of collisions between members of the two sets having their coordinates $p_1 \ldots . p_{n-1}$ and velocities $v_1 \ldots . v_n$.

Similarly, the number in unit time of collisions in the reverse direction is

$$\mathrm{F}'f' \, \mathrm{R} \, dp'_1 \ldots . dp'_{n-1} \, dv_1 \ldots . dv_{n-1} \, d\mathrm{R}.$$

In the Maxwell-Boltzmann distribution $\mathrm{F}f$, $\mathrm{F}'f'$ are functions of the kinetic energy only, and this being the same in the two states, $\mathrm{F}f = \mathrm{F}'f'$. And as many direct as reverse collisions take place in unit time, which insures the permanence of the distribution.

12. If $\mathrm{F}f \neq \mathrm{F}'f'$, then the number of systems of the first kind whose coordinates and velocities lie between

$$\begin{array}{c} p_1 \text{ and } p_1 + dp_1, \\ \&c., \\ v_1 \text{ and } v_1 + dv_1, \\ \&c., \end{array}$$

is increased per unit of time by collisions with the second set, having coordinates and velocities between

$$\begin{array}{c} p_{r+1} \text{ and } p_{r+1} + dp_{r+1}, \\ \&c., \\ v_{r+1} \text{ and } v_{r+1} + dv_{r+1}, \\ \&c., \end{array}$$

by the quantity

$$dp_1 \ldots . dv_r \, (\mathrm{F}'f' - \mathrm{F}f) \, \mathrm{R} \, dp_{r+1} \ldots . dv_{n-1} \, d\mathrm{R},$$

and by collision with systems m without restriction by the quantity

$$dp_1 \ldots . dv_r \iint \ldots . (\mathrm{F}'f' - \mathrm{F}f) \, \mathrm{R} \, dp_{r+1} \ldots . dv_{n-1} \, d\mathrm{R},$$

in which R is a function of $v_1 \ldots . v_n$ and the coordinates, and the integration includes all values of $p_{r+1} \ldots . v_{n-1}$ and R.

We will suppose now (see 13, *post*) that the number F is not increased or diminished by any means except by collision with systems m. If that be so,

$$\frac{dF}{dt} = \iint \ldots . (F'f' - Ff)\, R\, dp_1 \ldots . dp_{n-1}\, dv_1 \ldots . dv_{n-1}\, dR$$

and

$$\iint \ldots . \frac{dF}{dt} \log F\, dp_1 \ldots . dv_r$$

$$= \iint \ldots . (F'f' - Ff)\, R \log F\, dp_1 \ldots . dp_{n-1}\, dv_1 \ldots . dv_{n-1}\, dR.$$

By symmetry,

$$\iint \ldots . \frac{df}{dt} \log f\, dp_{r+1} \ldots . dv_n$$

$$= \iint \ldots . (F'f' - Ff)\, R \log f\, dp_1 \ldots . dp_{n-1}\, dv_1 \ldots . dv_{n-1}\, dR.$$

Now if

$$H = \iint \ldots . F (\log F - 1)\, dp_1 \ldots . dv_r + \iint \ldots , f (\log f - 1)\, dp_{r+1} \ldots . dv_n,$$

$$\frac{dH}{dt} = \iint \ldots . \frac{dF}{dt} \log F\, dp_1 \ldots . dv_r + \iint \ldots . \frac{df}{dt} \log f\, dp_{r+1} \ldots . dv_n,$$

and therefore

$$\frac{dH}{dt} = \iint \ldots . (F'f' - Ff)\, R \log (Ff)\, dp_1 \ldots . dp_n\, dv_1 \ldots . dv_n.$$

By symmetry, as we may interchange the accents,

$$\frac{dH}{dt} = \iint \ldots . (Ff - F'f')\, R \log (F'f')\, dp_1 \ldots . dp_n\, dv_1 \ldots . dv_n,$$

and therefore

$$\frac{dH}{dt} = \tfrac{1}{2} \iint \ldots . (F'f' - Ff)\, R \log \frac{Ff}{F'f'}\, dp_1 \ldots . dp_n\, dv_1 \ldots . dv_n,$$

which is necessarily negative, if not zero, and then only zero when $F'f' = Ff$, that is, when the Maxwell-Boltzmann distribution prevails.

13. It can be shown in the case of rigid elastic bodies that F is not

altered except by collisions, provided $f \log f$ and $F \log F$ become zero when any of the velocities becomes infinite.

14. The rate at which H approaches its minimum is found in the case of two sets of elastic spheres of masses M and m, whose numbers in unit volume are N and n, as follows:—Let $H = H_1 + K$, where H_1 is the minimum to which H tends. K is defined to be the *disturbance* and $\frac{1}{K}\frac{dK}{dt}$ the rate of subsidence.

Suppose that the number in unit volume of spheres M having velocities between U and $U+dU$ towards the element of volume $U^2\, dU \sin\alpha\, d\alpha\, d\beta$ is,

$$N\left(\frac{h\,.\,\overline{1+D}\,M}{\pi}\right)^{\frac{3}{2}} \epsilon^{-hM(1+D)U^2}\, U^2\, dU \sin\alpha\, d\alpha\, d\beta,$$

in which $h(1+D)$ is written for h in the usual expression for that number.

Similarly for the m spheres, $h(1+d)$ shall be written for h. It is assumed that the total energy is not altered by the disturbance, which requires that

$$\frac{N}{1+D} + \frac{n}{1+d} = N + n.$$

D and d are supposed so small that D^3 and d^3 are to be neglected.

Then we find $$K = \tfrac{3}{4}\frac{n}{N}\overline{N+n}\, d^2,$$

and $$\frac{dK}{dt} = -\frac{8}{\sqrt{\pi}}\frac{n}{N}(N+n)^2 \frac{\sqrt{(Mm)}}{(M+m)^{\frac{3}{2}}}\frac{\pi s^2}{\sqrt{h}}\, d^2,$$

where s is the sum of the radii of M and m.

Hence $$\frac{1}{K}\frac{dK}{dt} = -\frac{32}{3\sqrt{\pi}}\overline{N+n}\frac{\sqrt{(Mm)}}{(M+m)^{\frac{3}{2}}}\frac{\pi s^2}{\sqrt{h}} = -c,$$

and $$K = K_0\, \epsilon^{-ct},$$

$$d = d_0\, \epsilon^{-\frac{1}{2}ct}.$$

dK/dt is proportional to the density and to the square root of the absolute temperature.

IV. "On the Locus of Singular Points and Lines which occur in connexion with the Theory of the Locus of Ultimate Intersections of a System of Surfaces."

By M. J. M. HILL, M.A., Sc.D., Professor of Mathematics at University College, London. Communicated by Professor HENRICI, F.R.S.
Received October 5, 1891.

(Abstract.)

Introduction.

In a paper "On the c- and p-Discriminants of Ordinary Integrable Differential Equations of the First Order," published in vol. 19 of the 'Proceedings of the London Mathematical Society,' the factors which occur in the c-discriminant of an equation of the form $f(x, y, c) = 0$, where $f(x, y, c)$ is a rational integral function of x, y, c, are determined analytically.

It is shown* that if $E = 0$ be the equation of the envelope locus of the curves $f(x, y, c) = 0$; if $N = 0$ be the equation of their node locus; if $C = 0$ be the equation of their cusp locus; then the factors of the discriminant are E, N^2, C^3.

The singularities considered are those whose forms depend on the terms of the second degree only, when the origin of coordinates is at the singular point.

The object of this paper is to extend these results to surfaces.

It is well known that if the equation of a system of surfaces contain arbitrary parameters, and if a locus of ultimate intersections exist, then there cannot be more than two independent parameters.

Hence the investigation falls naturally into two parts: the first is the case where there is only one independent parameter, and the second is the case where there are two.

The investigation given in this paper is limited to the case in which the equation is rational and integral both as regards the coordinates and the parameters.

PART I.

The Equation of the Surfaces is a Rational Integral Function of the Coordinates and one Arbitrary Parameter.

In the case in which there is only one arbitrary parameter each surface of the system intersects the consecutive surface in a curve

* The theorem was originally given by Professor Cayley, in the 'Messenger of Mathematics,' vol. 2, 1872, pp. 6–12.

whose equations are the equation of the surface and the equation obtained by differentiating it with regard to the parameter. These equations will be called the fundamental equations in this part. Hence each surface touches the envelope along a curve, which is called a characteristic. It is known that the equation of the envelope may be obtained by eliminating the parameter from the fundamental equations and equating a factor of the result to zero. But it frequently happens that there are other factors of the result (or discriminant as it will in future be called) which when equated to zero do not give the equation of the envelope.

Following out the same line of argument as that used in reference to a system of plane curves, it will be shown that these factors are connected with loci of singular points. Now if each surface have one singular point, then its coordinates may in general be expressed as functions of the parameter of the surface to which it belongs. Hence the locus of all the singular points of the surfaces of the system is a curve. Its equations, therefore, cannot be found by equating a factor of the discriminant to zero. But if each surface of the system have upon it a nodal line, then the locus of the nodal lines of all the surfaces is a surface, and it will be shown that its equation may be found by equating to zero a factor of the discriminant.

The singular points in space, the form of which depends only on the terms of the second order, when the origin of coordinates is taken at the singular point, are:

(i.) The conic node, where all the tangent lines to the surface lie on a cone of the second order.

(ii.) The biplanar node or binode. This is the particular case of the preceding, in which the tangent cone to the surface breaks up into two non-coincident planes. These planes are called the biplanes, and their intersection is called the edge of the binode.

(iii.) The uniplanar node or unode. This is the particular case of the conic node, in which the tangent cone breaks up into two coincident planes. The plane with which these planes coincide is called the uniplane.

It is shown that a surface cannot have upon it a curve at every point of which there is a conic node. Hence there are two varieties of nodal lines to be considered; the first, being such that every point is a binode, may be called a binodal line; and the second, being such that every point on it is a unode, may be called a unodal line.

It will be proved that if $E = 0$ be the equation of the envelope locus, $B = 0$ the equation of the locus of binodal lines, $U = 0$ the equation of the locus of unodal lines, then the factors of the discriminant are in general E, B^2, U^3.

This is the general theorem, but it is assumed in the course of the investigation, when the discriminant is being formed, that the fundamental equations are satisfied by only one value of the parameter at each point on the envelope locus or on a locus of binodal or unodal lines.

The investigation is accordingly carried a step further, and it is shown that if the fundamental equations are satisfied by two equal values of the parameter at points on an envelope locus, or on a locus of binodal or unodal lines, the factors of the discriminant are E^2, B^3, U^4.

The geometrical meaning of the condition that the fundamental equations are satisfied by two equal values of the parameter in the case of the envelope is that the line of contact of the envelope with each surface of the system counts three times over as a curve of intersection, instead of twice as in the ordinary case, or that two consecutive characteristics coincide. The meaning of the condition in the case of the loci of singular lines is that each of these loci is also an envelope.

The results are given in the following table:—

Description of locus.	Factor of discriminant corresponding to locus.	Number of values of parameter satisfying fundamental equations.
Envelope	E (Art. 1)	1
Locus of binodal lines ..	B^2 (Art. 5)	1
Locus of unodal lines ...	U^3 (Art. 6)	1
Envelope such that two consecutive characteristics coincide.	E^2 (Art. 7)	2 coinciding
Locus of binodal lines, which is also an envelope.	B^3 (Art. 8)	2 coinciding
Locus of unodal lines, which is also an envelope.	U^4 (Art. 9)	2 coinciding

Part II.

The Equation of the System of Surfaces is a Rational Integral Function of the Coordinates and two Arbitrary Parameters.

In the case in which there are two arbitrary parameters in the equation of the system of surfaces, the equation of the locus of ulti-

mate intersections is found by eliminating the parameters between this equation and the two equations obtained by differentiating it with regard to the parameters. These equations will in this part of the investigation be called the fundamental equations.

In general the locus of ultimate intersections is a surface, for the coordinates of each point on it can be expressed as functions of the two arbitrary parameters. The exceptional cases in which it is not a surface are enumerated at the end of the paper. These include the case where the equation of the system of surfaces is of the first degree in the parameters. Hence it will be supposed that the degree of the equation of the system of surfaces in the parameters is above the first.

In general also the locus of ultimate intersections possesses the envelope property, and the equation of the envelope is determined by equating the discriminant, or a factor of it, to zero.

If factors of the discriminant exist which, when equated to zero, give surfaces not possessing the envelope property, then, as in Part I, it is shown that these surfaces are connected with loci of singular points.

Now the locus of singular points of a system of surfaces whose equation contains two arbitrary parameters is in general a curve (not a surface), whose equations can be obtained by eliminating the two parameters from the equation of the system of surfaces and the three equations obtained by differentiating it with regard to the coordinates. Hence its equations cannot be determined by equating to zero a factor of the discriminant.

But if every surface of the system have a singular point, then *in general* its coordinates may be expressed as functions of the two parameters of the surface to which it belongs. Hence the locus of the singular points is a surface. It will be proved that it is a part of the locus of ultimate intersections. Hence its equations can be obtained by equating to zero a factor of the discriminant.

Let now $E = 0$ be the equation of the envelope-locus,

$C = 0$ the equation of the conic node locus,

$B = 0$ the equation of the biplanar node locus,

$U = 0$ the equation of the uniplanar node locus.

Now at any point on the locus of ultimate intersections—

(I.) *There may be one system of values of the parameters satisfying the fundamental equations.*

In this case there may be envelope, conic node, or biplanar node loci; and the results are given in the following table :—

Description of locus.	Factor of discriminant corresponding to locus.	Nature of intersection of surfaces represented by fundamental equations at a point on locus of ultimate intersections.
Envelope	E (Art. 9)	1 point (Art. 26)
Conic node locus	C^2 (Art. 10)	2 points (Art. 27)
Biplanar node locus ...	B^3 (Art. 11)	3 points (Art. 28)

(II.) *There may be more than one system of distinct values of the parameters satisfying the fundamental equations.*

In this case the effect of the distinct values is additive. Thus if there be p systems of values at a point on the envelope locus, the factor E would occur to the p^{th} power.

(III.) *Two or more systems of values of the parameters satisfying the fundamental equations may coincide.*

The results must be stated differently in the cases (α) where the degree in the parameters of the equation of the system of surfaces is greater than two; (β) where the degree in the parameters of the equation of the system of surfaces is two.

In the case (α) it will be shown that there may be envelope-loci, in which the envelope has stationary contact with each surface of the system; conic-node loci, which are also envelopes; biplanar node loci, in which the edge always touches the biplanar node locus; and uniplanar node loci. The results are given in the following table:—

Description of locus.	Factor of discriminant corresponding to locus.	Nature of intersection of surfaces represented by fundamental equations at a point on locus of ultimate intersections
Envelope locus having stationary contact with each surface of system	E^2 (Art. 13)	2 points (Art. 26)
Conic node locus, which is also an envelope	C^3 (Art. 14)	3 points (Art. 27)
Biplanar node locus with edge of biplanar node touching biplanar node locus	B^4 (Art. 15)	4 points (Art. 28)
Uniplanar node locus	U^6 (Art. 12)	6 points (Art. 29)

The case (β) always falls under the next case.

(IV.) *The values of the parameters satisfying the fundamental equations may become indeterminate.*

If the equation of the system of surfaces be of the second degree in the parameters, and the analytical condition hold which expresses that the fundamental equations are satisfied by two coinciding systems of values, then this condition requires to be specially interpreted. For now the second and third fundamental equations are of the first degree in the parameters, so that if they are satisfied by two coinciding systems of values, they must be indeterminate.

It is, however, possible to determine a *single* system of values of the parameters satisfying them. In this case the three surfaces represented by the fundamental equations intersect in a common curve (which is fixed for fixed values of the parameters) lying on the locus of ultimate intersections; whereas in the previous cases they intersect in a finite number of points lying on the locus of ultimate intersections.

The surface of the system, corresponding to the fixed values of the parameters, touches the locus of ultimate intersections along the above-mentioned curve.

In general there are *two* conic nodes of the system at every point of the locus of ultimate intersections. The parameters of the surfaces having the conic nodes are determined by two quadratic equations, called the parametric quadratics; and in general the roots of *each* parametric quadratic are *unequal.* If the roots of *both* parametric quadratics are *equal,* the two surfaces having conic nodes are replaced by one surface having a biplanar or uniplanar node.

If the parameters of *one* of the surfaces having a conic node become *infinite,* this surface may be considered to disappear, and there is but one conic node at each point of the locus of ultimate intersections.

If the parameters of *both* surfaces having conic nodes become *infinite,* both these surfaces may be considered to disappear, and the locus of ultimate intersections is an envelope locus (touching each surface of the system along a curve).

If the parameters of *both* surfaces having conic nodes become *indeterminate,* then there are at each point an infinite number of biplanar nodes, and each surface of the system has a binodal line lying on the locus of ultimate intersections.

The results are given in the following table:—

Description of locus.	Factor of discriminant corresponding to locus.	Both parametric quadratics have
Locus of two conic nodes	C^2 (Art. 19)	Their roots unequal
Biplanar node locus with edge of biplanar node touching biplanar node locus.	B^3 (Art. 21)	Their roots equal
Uniplanar node locus....	U^4 (Art. 22)	Their roots equal
Locus of one conic node..	C^2 (Art. 23)	One root infinite
Envelope locus..........	E^3 (Art. 24)	Both roots infinite
Binodal line locus.......	B^4 (Art. 25)	Their roots indeterminate

It will be noticed that when the equation of the system of surfaces is of the second degree in the parameters, and the condition holds which expresses that the fundamental equations are satisfied by two coinciding values of the parameters, there is a reduction in the number of factors of the discriminant corresponding to the singular point loci, the factors C^3, B^4, U^6 becoming C^2, B^3, U^4 respectively.

The explanation is as follows :—

The discriminant is formed by solving the second and third fundamental equations for the parameters, substituting each pair of values in the left-hand side of the first fundamental equation, multiplying the results together, and also multiplying by a rationalising factor. Now in the case where the degree of the equation in the parameters is the second, there is only *one* system of roots corresponding to the loci under consideration, whereas there are *two* when the degree in the parameters is above the second. Hence this accounts for a diminution in the number of factors when the degree in the parameters is the second.

But this diminution is partly counterbalanced by an increase due to the fact that the rationalising factor vanishes at every point on the locus of ultimate intersections, and consequently increases the number of factors corresponding to the singular point loci. The result of the two causes is what has been stated above.

Presents, November 19, 1891.

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Williamson (B.), F.R.S. An Elementary Treatise on the Integral Calculus. Sixth edition. 8vo. *London* 1891. The Author.

Wolf (R.) Astronomische Mittheilungen. April, 1891. 8vo. [*Zürich.*] The Author.

Rectangular Bronze Medal struck in honour of M. G. A. Hirn. M. Grosseteste, Mülhausen.

November 26, 1891.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents was laid on the table, and thanks ordered for them.

Pursuant to notice, Alexander Agassiz, Dr. Benjamin Apthorpe Gould, Professor Eduard Strasburger, and Professor Pietro Tacchini were balloted for and elected Foreign Members of the Society.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows:—

President.—Sir William Thomson, D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— Professor Michael Foster, M.A., M.D.; The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Archibald Geikie, LL.D.

Other Members of the Council.—Captain William de Wiveleslie Abney, C.B.; William Thomas Blanford, F.G.S.; Professor Alexander Crum Brown, D.Sc.; Professor George Carey Foster, B.A.; James Whitbread Lee Glaisher, D.Sc.; Frederick Ducane Godman, F.L.S.; John Hopkinson, D.Sc.; Professor George Downing Liveing, M.A.; Professor Joseph Norman Lockyer, F.R.A.S.; Professor Arthur Milnes Marshall, D.Sc.; Philip Henry Pye-Smith, M.D.; William Chandler Roberts-Austen, F.C.S.; Professor Edward Albert Schäfer, M.R.C.S.; Sir George Gabriel Stokes, Bart., M.A.; Professor Sydney Howard Vines, M.A.; General James Thomas Walker, C.B.

The following Papers were read:—

I. "On Instability of Periodic Motion." By Sir WILLIAM THOMSON, P.R.S. Received November 12, 1891.

1. Let $\psi, \phi, \chi, \vartheta$ be generalised coordinates of a system; and let A $(\psi, \phi, \ldots, \psi', \Phi', \ldots)$ be the action in a path (§ 2 above) from the configuration $(\psi', \phi', \ldots)$ to the configuration $(\psi, \phi, \ldots)$ with kinetic energy $(E-V)$ with any given constant value for E, the total energy; V being the potential energy, of which the

value is given for every possible configuration of the system. Let $\nu, \xi, \eta, \zeta, \ldots,$ and $\nu', \xi', \eta', \zeta', \ldots,$ be the generalised component momentums of the system as it passes through the configurations $(\psi, \phi, \ldots)$ and $(\psi', \phi', \ldots)$ respectively. If by any means we have fully solved the problem of the motion of the system under the given forcive* (of which V is the potential energy), we know A for every given set of values of $\psi, \phi, \ldots, \psi', \phi', \ldots,$ that is to say, it is a known function of $(\psi, \phi \ldots, \psi', \phi', \ldots)$. Then, by Hamilton's principle [Thomson and Tait's 'Natural Philosophy,' § 330 (18)], we have

$$\left.\begin{array}{llll} \nu = \dfrac{dA}{d\psi}, & \xi = \dfrac{dA}{d\phi}, & \eta = \dfrac{dA}{d\chi}, & \zeta = \dfrac{dA}{d\vartheta}, \ldots \\[2ex] \nu' = -\dfrac{dA}{d\psi'}, & \xi' = -\dfrac{dA}{d\phi'}, & \eta' = -\dfrac{dA}{d\chi'}, & \zeta' = \dfrac{dA}{d\vartheta'}, \ldots \end{array}\right\} \ldots (1).$$

2. Now let P′P designate a particular path † from position $(\psi', \phi', \chi', \ldots)$ which for brevity we shall call P′, to position $(\psi, \phi, \chi, \ldots)$ which we shall call P. Let ${}_0P'{}_0P$ be a part of a known periodic path, from which P′P is evidently little distant. But first, whether ${}_0P'{}_0P$ is periodic or not, provided it is evidently near to P′P, and provided ${}_0P'$ and ${}_0P$ are infinitely near to P′, and P, respectively, we have, by Taylor's theorem, and by (1),

$$\left.\begin{array}{l} A(\psi, \phi, \chi, \ldots, \psi', \phi, \chi, \ldots) \\ = A({}_0\psi, {}_0\phi, {}_0\chi, \ldots, {}_0\psi', {}_0\phi', {}_0\chi', \ldots) \\ \quad + {}_0\nu(\psi - {}_0\psi) + {}_0\xi(\phi - {}_0\phi) + \ldots - {}_0\nu'(\psi' - {}_0\psi') - {}_0\xi'(\phi' - {}_0\phi') - \ldots \\ \quad + \tfrac{1}{2}\left\{ {}_0\!\left(\dfrac{d^2A}{d\psi^2}\right)(\psi - {}_0\psi)^2 + {}_0\!\left(\dfrac{d^2A}{d\phi^2}\right)(\phi - {}_0\phi)^2 + \ldots \right. \\ \quad \left. + 2\,{}_0\!\left(\dfrac{d^2A}{d\psi\, d\phi}\right)(\psi - {}_0\psi)(\phi - {}_0\phi) + \ldots \right\} \end{array}\right\} \ldots (2).$$

* This is a term introduced by my brother, Professor James Thomson, to denote a force-system.

† For any given value of E, the sum of potential and kinetic energies, the problem of finding a path from any position P′ to any position P is determinate. Its solution is, for each coordinate of the system, a determinate function of the coordinates which define P and P′ and of t, the time reckoned from the instant of passing through P′. The solution is single for the case of a particle moving under the influence of no force; every path being an infinite straight line. For a single particle moving under the influence of a uniform force in parallel lines (as gravity in small-scale terrestrial ballistics) the solution is duplex or imaginary. For every constrainedly finite system the solution is infinitely multiple; as is virtually well known by every billiard player for the case of a Boscovichian atom flying about within an enclosing surface, and by every tennis player for the parabolas with which he is concerned, and their reflexions from walls or pavement.

3. Let us now simplify by choosing our coordinates so that the values of ϕ, χ, &c., are each zero for every position of the path ${}_0\text{P}'{}_0\text{P}$; and let ψ, for any position of this path, be the action along it reckoned from zero at ${}_0\text{P}'$. These assumptions, expressed in symbols, are as follows:—

$$\left.\begin{array}{l}\dfrac{d\text{A}}{d\phi}=0,\quad \dfrac{d\text{A}}{d\chi}=0,\ldots \quad \dfrac{d\text{A}}{d\psi}=-\psi',\quad \dfrac{d\text{A}}{d\phi'}=0,\quad \dfrac{d\text{A}}{d\chi'}=0,\ldots \\ \text{for all values of } \psi \text{ and } \psi', \text{ if } \phi=0, \chi=0,\ldots;\ \phi'=0, \chi'=0,\ldots\end{array}\right\} \quad ..(3).$$

4. Taking now

$$\psi=0,\ \psi={}_0\psi,\ {}_0\phi=0,\ {}_0\chi=0,\ldots,\ {}_0\psi'=0,\ {}_0\phi'=0,\ {}_0\chi'=0,\ldots, \quad ..(4);$$

we have

$$\text{A}({}_0\psi, {}_0\phi, {}_0\chi, \ldots, {}_0\psi', {}_0\phi', {}_0\chi', \ldots) = \text{A}({}_0\psi, 0, 0, \ldots, 0, 0, 0, \ldots) \quad (5)$$

and, in virtue of this and of (3) and (1), (2) becomes

$$\left.\begin{array}{r}\text{A}({}_0\psi, \phi, \chi, \ldots, 0, \phi', \chi', \ldots) = \text{A}({}_0\psi, 0, 0, \ldots, 0, 0, 0) \\ +\tfrac{1}{2}\,[11\phi^2+22\chi^2+33\vartheta^2+44\phi'^2+55\chi'^2+66\vartheta'^2 \\ +2(12\phi\chi+13\phi\vartheta+14\phi\phi'+15\phi\chi'+16\phi\vartheta' \\ 23\chi\vartheta+24\chi\phi'+25\chi\chi'+26\chi\vartheta' \\ +34\vartheta\phi'+35\vartheta\chi'+36\vartheta\vartheta' \\ +45\phi'\chi'+46\phi'\vartheta' \\ +56\chi'\vartheta')]\end{array}\right\} \quad ..(6),$$

where, merely for simplicity of notation, we suppose the total number of freedoms of the system, that is to say, the total number of the coordinates ψ, ϕ, χ, ϑ, to be four; and for brevity put

$$_0\!\left(\frac{d^2\text{A}}{d\phi^2}\right)=11,\quad {}_0\!\left(\frac{d^2\text{A}}{d\phi\,d\chi}\right)=12,\quad {}_0\!\left(\frac{d^2\text{A}}{d\chi^2}\right)=22, \text{ \&c.} \quad \ldots\ldots(7).$$

5. From (6) we find, by (1),

$$\left.\begin{array}{r}\xi=11\phi+12\chi+13\vartheta+14\phi'+15\chi'+16\vartheta' \\ \eta=21\phi+22\chi+23\vartheta+24\phi'+25\chi'+26\vartheta' \\ \zeta=31\phi+32\chi+33\vartheta+34\phi'+35\chi'+36\vartheta' \\ -\xi'=41\phi+42\chi+43\vartheta+44\phi'+45\chi'+46\vartheta' \\ -\eta'=51\phi+52\chi+53\vartheta+54\phi'+55\chi'+56\vartheta' \\ -\zeta'=61\phi+62\chi+63\vartheta+64\phi'+65\chi'+66\vartheta'\end{array}\right\} \ldots\ldots\ldots (8).$$

These equations allow us to determine the three displacements, ϕ, χ, ϑ, and the three corresponding momentums, ξ, η, ζ, for any position on the path, in terms of the initial values, $\phi', \chi', \vartheta', \xi', \eta', \zeta'$, supposed known.

6. To introduce now our supposition (§ 2) that ${}_0P'{}_0P$ is part of a periodic path; let Q be a position on it between ${}_0P'$ and ${}_0P$; and let us now, to avoid ambiguity, call it ${}_0P'Q_0P$. Let ${}_0P'$ and ${}_0P$ now be taken to coincide in a position which we shall call O; in other words, let ${}_0P'Q_0P$, or OQO, be the complete periodic circuit, or orbit as we may call it. Our path P′P is now a path infinitely near to this orbit, and P′ and P are two consecutive positions in it for which ψ has the value zero. These two positions are infinitely near to one another and to O. We shall call them O_i, and O_{i+1}, considering them as the positions on our path in which ψ is zero for the ith time and for the $(i+1)$th time, from an earlier initial epoch than first passage through $\psi = 0$ which we have been hitherto considering. It is accordingly convenient now to modify our notation as follows:—

$$\left.\begin{array}{llllll} \phi' = \phi_i, & \chi' = \chi_i, & \vartheta' = \vartheta_i; & \xi' = \xi_i, & \eta' = \eta_i, & \zeta' = \zeta_i \\ \phi = \phi_{i+1}, & \chi = \chi_{i+1}, & \vartheta = \vartheta_{i+1}; & \xi = \xi_{i+1}, & \eta = \eta_{i+1}, & \zeta = \zeta_{i+1} \end{array}\right\} \ldots (9).$$

Here $\phi_i, \chi_i, \vartheta_i$ are the generalised components of distance from O, at the ith transit through $\psi = 0$ of the system pursuing its path infinitely near to the orbit; and ξ_i, η_i, ζ_i are the corresponding momentum components. With the notation of (9), equations (8) become equations by which the values of these components for the $i+1$th time of transit through $\psi = 0$ can be found from their values for the ith time. They are equations of finite differences, and are to be treated *secundum artem*, as follows:—

7. Assume

$$\left.\begin{array}{lll} \phi_{i+1} = \rho\phi_i, & \chi_{i+1} = \rho\chi_i, & \vartheta_{i+1} = \rho\vartheta_i; \\ \xi_{i+1} = \rho\xi_i, & \eta_{i+1} = \rho\eta_i, & \zeta_{i+1} = \rho\zeta_i \end{array}\right\} \ldots\ldots\ldots (10).$$

Substituting accordingly in (8) modified by (9), and eliminating ξ_i, η_i, ζ_i, we find

$$\left.\begin{array}{l} \left(11+\frac{14}{\rho}+41\rho+44\right)\phi+\left(12+\frac{15}{\rho}+42\rho+45\right)\chi+\left(13+\frac{16}{\rho}+43\rho+46\right)\vartheta = 0 \\ \left(21+\frac{24}{\rho}+51\rho+54\right)\phi+\left(22+\frac{25}{\rho}+52\rho+55\right)\chi+\left(23+\frac{26}{\rho}+53\rho+56\right)\vartheta = 0 \\ \left(31+\frac{34}{\rho}+61\rho+64\right)\phi+\left(32+\frac{35}{\rho}+62\rho+65\right)\chi+\left(33+\frac{36}{\rho}+63\rho+66\right)\vartheta = 0 \end{array}\right\} (11).$$

Remarking that $41 = 14$, $12 = 21$, &c., we see that the determinant

for the elimination of the ratios $\phi|\chi|\vartheta$ is symmetrical with reference to ρ and $1|\rho$. Hence it is

$$C_3(\rho^3+\rho^{-3})+C_2(\rho^2+\rho^{-2})+C_1(\rho+\rho^{-1})+2C_0 \dots\dots\dots (12),$$

where C_0, C_1, C_2, C_3 are coefficients of which the values in terms of 11, 12, &c., are easily written out. The determinant equated to zero gives an equation of the sixth degree for determining ρ, of which for each root there is another equal to its reciprocal. We reduce it to an equation of the third degree by putting

$$\rho+\rho^{-1} = 2e \dots\dots\dots\dots (13).$$

Let e_1, e_2, e_3 be the roots of the equation thus found. The corresponding values of ρ are

$$e_1 \pm \surd(e_1^2-1); \quad e_2 \pm \surd(e_2^2-1); \quad e_3 \pm \surd(e_3^2-1) \dots\dots (14).$$

In the case of e having any real value between 1 and -1, it is convenient to put

$$\left.\begin{array}{lr} & e = \cos\alpha, \\ \text{which gives} & \rho = \cos\alpha+\iota\sin\alpha \\ \text{and} & \rho^{-1} = \cos\alpha-\iota\sin\alpha \end{array}\right\} \dots\dots\dots\dots (15).$$

8. Suppose now, for the first time of passing through $\psi = 0$, the three coordinates and three corresponding momentums, ϕ_1, χ_1, ϑ_1, ξ_1, η_1, ζ_1, to be all given; we find

$$\left.\begin{array}{l} \phi_{i+1} = A_1\rho_1{}^i+A'_1\rho_1{}^{-i}+A_2\rho_2{}^i+A'_2\rho_2{}^{-i}+A_3\rho_3{}^i+A'_3\rho_3{}^{-i} \\ \chi_{i+1} = B_1\rho_1{}^i+B'_1\rho_1{}^{-i}+B_2\rho_2{}^i+B'_2\rho_2{}^{-i}+B_3\rho_3{}^i+B'_3\rho_3{}^{-i} \\ \quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\ \quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot \\ \xi_{i+1} = F_1\rho_1{}^i+F'_1\rho_1{}^{-i}+F_2\rho_2{}^i+F'_2\rho_2{}^{-i}+F_3\rho_3{}^i+F'_3\rho_3{}^{-i} \end{array}\right\} \dots (16),$$

where A_1, A'_1, A_2, A'_2, ..., F_1, F'_1, F_2, F'_2 are thirty-six coefficients which are determined by the six equations (16), with $i = 0$: and the six equations (8), modified by (9); with i successively put = 1, 2, 3, 4, 5; with the given values substituted for ϕ_1, χ_1, ϑ_1, ξ_1, η_1, ζ_1 in them; and with for ϕ_2, χ_2, &c., their values by (16).

9. Our result proves that every path infinitely near to the orbit is unstable unless every root of the equation for e has a real value between 1 and -1. It does not prove that the motion *is* stable when this condition is fulfilled. Stability or instability for this case cannot be tested without going to higher orders of approximation in the consideration of paths very nearly coincident with an orbit.

ADDENDUM.

The subject of periodic motion and its stability has been treated with great power by M. Poincaré in a paper, "Sur le Problème des Trois Corps et les Équations de la Dynamique," for which the prize of His Majesty the King of Sweden was awarded on the 21st January, 1889. This paper, which has been published in Mittag-Leffler's 'Acta Mathematica,' 13, 1 and 2 (270 4to pp.), Stockholm, 1890, only became known to me recently through Professor Cayley. I am greatly interested to find in it much that bears upon the subject of my communication of last June to the Royal Society "On some Test Cases for the Maxwell-Boltzmann Doctrine regarding Distribution of Energy;" particularly in p. 239, the following paragraph :—"On peut démontrer que dans le voisinage d'une trajectoire fermée représentant une solution périodique, soit stable, soit instable, il passe une infinité d'autres trajectoires fermées. Cela ne suffit pas, en toute rigueur, pour conclure que toute région de l'espace, si petite qu'elle soit, est traversée par une infinité des trajectoires fermées, mais cela suffit pour donner à cette hypothèse un haut caractère de vraisemblance."* This statement is exceedingly interesting in connexion with Maxwell's fundamental supposition quoted in § 10 of my paper, "that the system, if left to itself in its actual state of motion, will, sooner or later, pass through every phase which is consistent with the equation of energy;"† an assumption which Maxwell gives not as a conclusion, but as a proposition which "we may with considerable confidence assert, ... except for particular forms of the surface of the fixed obstacle." It will be seen that Poincaré's "hypothesis, having a high character of probability," does not go so far as Maxwell's, which asserts that every portion of space is traversed *in all directions* by *every trajectory.* The conclusion which I gave in § 13, as seeming to me quite certain, "that every mode differs infinitely little from being a fundamental mode," is clearly a necessary consequence of Maxwell's fundamental supposition; the truth of which still seems to me highly probable, *provided exceptional cases are properly dealt with.*

I also find the following statement, pp. 100—101 :—"Il y aura donc en général n quantités α^2 distinctes. Nous les appellerons les *coefficients de stabilité* de la solution périodique considérée.

"Si ces n coefficients sont tous réels et négatifs, la solution périodique sera stable, car les quantités ξ_i and η_i, resteront inférieures à une limite donnée.

"Il ne faut pas toutefois entendre ce mot de stabilité au sens

* The "trajectoire fermée" of M. Poincaré is what I called a "fundamental mode of rigorously periodic motion," or "an orbit."

† 'Scientific Papers,' vol. 2, p. 714.

absolu. En effet, nous avons négligé les carrés des ξ et des η et rien ne prouve qu'en tenant compte de ces carrés, le résultat ne serait pas changé. Mais nous pouvons dire au moins que les ξ et η, s'ils sont originairement très petits, resteront très petits pendant très longtemps. Nous pouvons exprimer ce fait en disant que la solution périodique jouit sinon de la stabilité *séculaire*, du moins de la stabilité *temporaire*." Here the conclusion of § 9 of my present paper is perfectly anticipated, and is expressed in a most interesting manner. M. Poincaré's investigation and mine are as different as two investigations of the same subject could well be, and it is very satisfactory to find perfect agreement in conclusions.

II. "A new Mode of Respiration in the Myriapoda." By F. G. Sinclair (formerly F. G. Heathcote), M.A., Fellow of the Cambridge Philosophical Society. Received August 12, 1891.

(Abstract.)

The Scutigeridæ respire by means of a series of organs arranged in the middle dorsal line at the posterior edge of every dorsal scale except the last.

Each organ consists of a slit bounded by four curved ridges, two at the edges of the slit, and two external to the latter. The slit leads into an air sac. From the sac a number of tubes are given off; these tubes are arranged in two semicircular masses. The ends of the tubes project into the pericardium in such a manner that the ends are bathed in the blood and aërate it just before it is returned into the heart by means of the ostia. In the living animal the blood can be seen through the transparent chitin of the dorsal surface surrounding the ends of the tubes; and in the organ and surrounding tissues cut out of a Scutigera directly it is killed, the blood corpuscles can be seen clustering round the tube ends. If the mass of tubes of a freshly killed specimen are teased out under the microscope in glycerine, they can be seen to be filled with air. The tubes each branch several times. Each tube is lined with chitin, which is a continuation of the chitin of the exo-skeleton. Each tube is also clothed with cells, which are a continuation of the hypodermis. The tubes end in a blunt point of very delicate chitin.

Reasons for supposing these Organs to be Respiratory.

1. There are no other organs which could be supposed to be respiratory in function.

2. The tubes are chitinous, and the chitin grows thin and mem-

branous towards the end, affording a good opportunity for interchange of gases.

3. The tube ends project into the pericardium, so that they are bathed with the blood.

4. The tubes are filled with air.

5. The organ is so placed as to aërate the blood just before it returns to the heart.

6. In Scutigera the dorsal scales do not agree in number with the legs. The organs are arranged on the dorsal scales; that is they are not arranged in correspondence with the mesoblastic or primitive segmentation (see a former paper before this Society, "The Post-Embryonic Development of *Julus terrestris*," 1888). This renders it probable that they are not a primitive development, but a recent modification, agreeing with the fact that all other Myriapods breathe by the more primitive method of tracheæ.

This mode of respiration differs from that in other Myriapods in the following particulars:—

1. The tubes are collected into one definite organ, instead of being distributed about the body.

2. The tubes have no spiral thread.

3. In acting on the blood just before it returns to the heart, so that aërated blood is distributed instead of unaërated.

It resembles the tracheæ of other Myriapods in the following particulars:—

1. In the air sac into which the tubes open.

2. In the cylindrical form of the tubes.

3. In the branching of the tubes.

The organs resemble the tracheal lungs of Spiders—

1. In the large air sac.

2. In the number of tubes opening into an air sac.

3. In the arrangement for bathing the tubes with blood in a blood sinus.

4. In the supply of aërated blood by the heart.

They differ from them in—

1. The form of the tubes, which in Scutigera are cylindrical.

2. In the absence of the membrane which in Spiders surrounds the organ.

I therefore hold that the respiratory organ in Scutigera holds a position intermediate between the tracheæ of Myriapods and the lungs of Spiders. I hold with A. Leuckart ('Zeitsch. für Wiss. Zool.,' vol. 1, p. 246, 1849, "*Ueber den Bau und Bedeutung der sog. Lungen bei den Arachniden*") that the tracheæ have developed into the lungs of Spiders and Scorpions, and I think that the organs in question form a series of which the lowest term is the tracheæ, the next the organ of Scutigera, then the lungs of Spiders, and then of Scorpions.

III. "Further Observations on the Gestation of Indian Rays: being Natural History Notes from H.M. Indian Marine Survey Steamer 'Investigator,' Commander R. F. Hoskyn, R.N., Commanding. Series II. No. 2." By J. Wood-Mason, Superintendent of the Indian Museum, and Professor of Comparative Anatomy in the Medical College of Bengal, and A. Alcock, M.B., Surgeon, I.M.S., Surgeon-Naturalist to the Survey. Communicated by Professor M. Foster, Sec. R.S. Received October 9, 1891.

Contents.

§ 1. *Introduction.*

On the 24th of February of this year we communicated the results of some observations on the uterine villiform papillæ, or trophonemata, of *Pteroplatœa micrura* (Bl. Schn.) and their relation to the embryo, and also incidentally referred to the structure and probable functions of similar uterine papillæ in *Trygon bleekeri* and *Myliobatis nieuhofii*.

We were able to show that in the pregnant females of these Batoids the mucous membrane of the uterus is extended in the form of elongate papillæ, the entire surface of which, again, is beset with tubular glands, and we were able to bring forward evidence in favour of the view—more especially in the case of *Pteroplatœa micrura*—that the function of these glands is to secrete a nutritive fluid which is conveyed down the pharynx and into the stomach of the embryo.

Since the date of that communication we have been fortunate enough to obtain, in the course of the "Investigator's" survey of the Godávari Delta, pregnant and parturient females of *Trygon walga*, Müller and Henle, a pregnant female of *Myliobatis nieuhofii* (Bl. Schn.), numerous pregnant females of *Pteroplatœa micrura* (Bl. Schn.), and an unimpregnated uterus of *Narcine*, the examination of which appears to corroborate the view advanced by us as to the function of the uterine papillæ and their relation to the embryo.

In the case of *Pteroplatœa micrura*, of which species we have examined over a score of pregnant females, we find that the oviduct is dilated into an uterus on both sides of the body equally, and that each uterus may contain from one to three embryos, the usual number being two. In the early stages of gestation the entire surface of the

mucous membrane of the uterus is beset with trophonemata, but with the growth of the fœtus the trophonemata become atrophied by pressure, except opposite the spiracles of the fœtus, where they remain as large bunches which penetrate through the spiracles deep into the fœtal pharynx. We find that where, as appears to be the most common condition, two embryos occupy the uterus, the one is rolled up within the other—head to head and tail to tail—but in such a way as to leave the spiracles of the inner one exposed for the trophonemata to enter. In one remarkable instance, where one spiracle of the inner embryo was overlapped and concealed, that spiracle was of diminutive size, while its fellow of the opposite side was much enlarged. On microscopic examination we find the nutrient secretion of the trophonemata to contain numerous small granular bodies, and a few large granular corpuscles which resemble leucocytes.

In the pregnant female of *Myliobatis nieuhofii* we found three young ones, two males and a female, of different sizes, in an uterine enlargement which exists on the left side only. In the two smallest there are very delicate external gills; but of such structures there is no trace whatever in the largest embryo.

The spiracles in all are singularly large and patent, being kept open by an eave-like extension of the cranial cartilage. The mucous membrane of the uterus, which is thickly beset with long branched glandular villi, was intensely vascular, had an odour, not of fish, but like that of raw beef, and was covered with a creamy yellowish-coloured fluid somewhat resembling "laudable pus" in appearance. It may be mentioned that the spiral gut of one of the embryos was found to be full of the same creamy fluid, unchanged. Apparently loose in the body cavity, in the largest fœtus, were found strings of bead-like concretions held together by inspissated albuminous material: and on examination under the microscope these concretions were found to contain numerous crystals of oxalate of lime and dumb-bell-shaped bodies exactly resembling the dumb-bell concretions of urate of ammonia found in human urine.

As one of us has already described ('Journal Asiatic Society Bengal,' vol. 59, Part II, pp. 54 and 55) the general structure of the uterus and uterine glands in the stage preparatory to pregnancy in *Myliobatis nieuhofii*, we are deferring a report on this species in the hope of obtaining ampler material to make the report more complete.

We devote this paper to some account of the phenomena of gestation in *Trygon walga*, in which we have been able to make fuller observations.

§ 2. *On the Parturient Female and New-born Young of* Trygon walga.

Two females of *Trygon walga*, Müller and Henle, were taken at Cocanáda, in the Godávari Delta, on the 8th of April of this year. The abdomen, in one of them, was so much distended that the normal flat shape of the fish was obscured. On transfer of this specimen to a bucket of sea-water, two young ones were seen to be suddenly extruded from the cloacal orifice—one a few minutes after the other. The young ones swam about vigorously in the bucket. Relatively to the size of the abdominal cavity of the mother, which in this species is much contracted, the young ones are enormous; and on seeing the two of them beside the open maternal abdomen it appears almost incredible that they could ever have been compressed into such a confined space.

The following are the measurements of the mother and offspring, with their weight after preservation in spirit:—

	Mother.	New-born young one, No. 1.	New-born young one, No. 2.
Length of disk	205 mm.	67 mm.	67 mm.
Breadth of disk	185 ,,	70 ,,	72 ,,
Length of tail	180 ,,	85 ,,	95 ,,
Length of snout from margin of fronto-nasal process to tip	57 ,,	19 ,,	19 ,,
Weight	2734 grains	200 grains	174 grains

Both young ones were females; neither of them had any traces of external gill filaments; both of them had a small papilliform umbilical vesicle about 1 mm. long. The spiracles in both were large and widely open.

Before going on to describe the visceral anatomy of the young, and the structure of the uterus and its trophonemata in the adult, there are certain interesting external characters, distinguishing the young from the mature female, which are important enough to be mentioned.

(1.) In the young, the dorsal surface of the body is quite smooth and devoid of the dermal tubercles which, in this situation, characterise the adult. (2.) The young have only one tail spine, whereas the adult has two, and in front of them a long series of fixed spinelets. (3.) The middle third of the space between the base of the tail spine and the tip of the tail is occupied in the young one by a median fold

of the dorsal integument 15 mm. long and about 0·75 mm. in height; this is, no doubt, a vestige of the vertical system of fins, of which no trace exists in the adult. (4.) The ventral fins in the young are placed relatively farther back than they are in the adult, projecting considerably beyond the hinder margin of the disk, while in the adult they barely reach this limit. (5.) The tail is relatively a good deal longer in the young.

The young are pigmented in all respects like the adult.

In the young one, immediately after it has left the uterus, the abdomen is very tumid, its anterior wall being stretched so thin that the abdominal and intestinal contents can be seen through it. On opening the abdomen, its cavity is found to be almost completely filled by the enormously distended colon (spiral gut), the liver, stomach, and duodenum being displaced forward beneath the pectoral girdle; the rectum is very sharply marked off from the colon, and appears as a narrow cord; the œsophagus and stomach are empty, but the colon is filled with bile-stained granular material, which, under the action of spirit, has become a hard, yellowish-brown cake, of which the weight is no less than one-sixth to one-seventh of that of the entire body.

The large relative size of the rectal gland is remarkable, not only in this species, but also, we may mention, in the fœtus of *Myliobatis nieuhofii* and *Pteroplatœa micrura*.

§ 3. *On the Uterus and Trophonemata of* Trygon walga *at the Close of Pregnancy.*

The abdomen of the mother was laid open immediately after the birth of the young ones. The right ovary and oviduct are undeveloped; the left ovary is large, and the distal end of its oviduct is dilated into a pyriform uterine swelling, the aperture of which projects into the cloaca as a conspicuous *os uteri*. On opening the uterus, which is much contracted when empty, we find a moderately thick fibrous and muscular wall, lined internally with a mucous membrane which is everywhere produced into long papillæ (trophonemata); these are brittle and friable.

The trophonemata, which are cylindrical, unbranched, and taper slightly from base to apex, are about 10 mm. in length and about 1 mm. in breadth; and, on examination with a low magnifying power, their surface is seen to be granular and much fissured.

A transverse section through the uterine wall shows, from without inwards, (1) a thin layer of fibrous tissue, (2) a layer of muscular fibres cut transversely, (3) a layer of muscular fibres cut longitudinally, and (4) the vascular submucosa and the mucosa about to be described as they appear in a trophonema.

Sections, both transverse and longitudinal, through a trophonema display a central core of fine connective tissue, in which, besides arteries, veins, and a dense capillary plexus, are cells and very numerous free leucocytes; and surrounding or external to this a series of solid finger-shaped coagula formed of confluent cells in various stages of degeneration. At the one extreme, these finger-shaped masses are seen to be made up of desquamated cells the protoplasm of which has simply become confluent into a solid mass wherein the nuclei, with the nuclear network very clear and conspicuous, stand out distinct and unchanged; while, at the other extreme, are found nothing but solid granular coagula in which neither nuclei nor structure of any kind can be distinguished, with leucocytes scattered between them.

We may anticipate events by stating that these solid masses of cells and granular coagula appear to be "epithelial casts" of the glands which, as we shall show, invest the surface of the trophonemata in the earlier stages of pregnancy, but which, at the close of pregnancy, are undergoing coagulative degeneration, while the leucocytes present appear to be exercising a resorptive function.

§ 4. *On the Uterus and Trophonemata of* Trygon walga *at the Beginning of Pregnancy.*

The second female specimen is about the same size as the first, but had not the same convexity of the abdomen. It also has the right ovary and oviduct undeveloped, while the left ovary is enlarged and distended with ova, and the distal end of its oviduct is dilated into a pyriform uterine chamber, which, however, is smaller than in the first specimen—measuring only 32 mm. from the fundus to the os. An egg which had recently descended into the uterus was ruptured accidentally in dissection.

The uterus has at this stage in all respects the same form as it has at the end of pregnancy; but the trophonemata, on the contrary, are as strongly contrasted as possible in the two stages. For while in the late stage they have the form of an elongated cone and are in process of disintegration, in the early stage they are strap-shaped and are in process of integration. That the trophonemata were not functioning, their incompletely evolved condition proves, apart from the consideration that in the well-filled yolk-sac there would be abundant sustenance for the embryo for some time.

The trophonemata are somewhat wavy, rather thick and fleshy, strap-shaped bodies, measuring about 10·5 mm. in length by about 1 mm. in greatest breadth. Narrow at their origin, they almost immediately widen out to their greatest breadth, which is maintained to about the seventh tenth of their length, whence they taper to their

rounded extremity. They are with difficulty straightened out from their wavy curl. In stained preparations a dark unbranched line is seen following the sinuosities of the trophonema, nearer to one margin than to the other, and tapering away to nothing at the apex; it is the rounded thickening formed by the (developing) axial vein which is so conspicuous in the trophonemata of *Pteroplatæa.*

A trophonema, lightly stained with borax carmine, mounted in spirit and glycerine, and viewed under a Zeiss D ocular 2 by transmitted light, shows a darker and broader median band and two lighter and much narrower marginal bands. In the former the axial vein presents itself as a streak with a pale axis, which transverse sections prove to be the optic expression of the commencing lumen. In each of the latter a large vessel, which in transverse section is seen to be an artery, can readily be made out. By careful focussing, the surface of the broad median band is seen to be covered with a coarse polygonal network, with paler meshes, which correspond to the simple or compound duct-openings of subjacent glands; the network, like the surface of the pale marginal bands, is covered continuously with minute flat glassy cells having sharply defined nuclei and distinct limiting membranes. Some preparations suggest that, in trophonemata which are less advanced in development than the one under description, this layer of pavement cells may form a continuous investment over the whole trophonema.

Transverse sections of a trophonema show that the meshes of the polygonal network above referred to coincide with bulb-shaped nests of cells which are the still solid foundations of glands.

These glands are arranged, as in *Pteroplatæa* (*vide* 'Roy. Soc. Proc.,' vol. 49, Pl. VIII, fig. 5), perpendicular to the surface, side by side, in the substance of the mucosa, but not quite so close together. They are squat bulb-shaped organs, and are often compound, with two, three, or, perhaps, four acini. In consequence of the absence of limiting cell-membranes, the exact arrangement of the gland cells cannot be made out, but the nuclei, which are elongate, are arranged somewhat irregularly in two strata—the nuclei of the two strata and of the same stratum overlapping one another; whence it may be inferred, though no cell-boundaries are traceable, that the gland tissue forms a two-layered stratified columnar epithelium similar to that which is found in many parts of the Mammalian respiratory tract.

The glands, which, as above stated, are quite solid without any trace of lumen, appear to have originated as ingrowths of the indifferent layers of the epithelium alone, the outer layer of flat glassy pavement cells which invests the surface of the trophonema between the glands and at its sides not having been involved in the process. And, from appearances presented by less developed trophonemata,

we infer that, in early stages, the pavement layer forms a continuous investment over the whole trophonema; and that subsequently, by the separation from one another at definite spots of the pavement cells, stomata are formed, which, when the lumina of the glands are established, become the mouths of the glands. Be this as it may, the outer (pavement) layer is not traceable into the mouths of the glands at any point in any of our sections.

The trophonemata possess an exceedingly rich vascular supply. All the sections show two large, but not very thick-walled, arteries, one in each non-glandular margin; minute arterioles here and there between adjacent glands immediately beneath the epithelium; and, between the layers of glands of opposite faces, a plexus of sinuous cavities or capillaries.

The capillary plexus in transverse sections of a trophonema is seen to extend deeply between the glands right up to the superficial arterioles; and, in the part corresponding to the position of the great axial vein of *Pteroplatœa*, it presents a solid or spongy circular expansion, in the centre of which the future lumen of the vessel is commencing to be formed. In transverse section of a group of glands the capillary plexus is seen to form a polygonal network, in the meshes of which the glands lie.

In comparison with *Pteroplatœa micrura*, the trophonemata of *Trygon walga* are characterised by the possession of an epithelium which is several cells thick instead of one cell thick, and by the richer vascular supply of their glands, each of which is embedded in a little capillary cup of its own, like a filbert in its husk.

§ 5. *Conclusions.*

1. Comparison of the trophonemata in the two stages above described, showing, on the one hand, at the onset of pregnancy, a mucous membrane of large nucleated indifferent cells and of solid unformed glands, and, on the other hand, when the term of pregnancy is fulfilled, a surface layer of gland-casts of epithelium in various stages of degeneration, appears to be conclusive proof that the glands are developed for the special requirements of the pregnant state.

2. As regards the function of these glands, the presence in every case where a fresh pregnant uterus has been examined of a viscid turbid or actually milky albuminous fluid, and further the finding in the case of *Myliobatis nieuhofii* of one and the same secretion in the uterus of the mother and in the intestine of the fœtus, seem to fully confirm our original conclusion that they are in all cases milk-glands furnishing a secretion for the nourishment of the embryo.

3. Regarding the channel through which the milk is carried into the fœtus, we think it to be in every species that we have examined,

Pteroplatœa micrura, *Myliobatis nieuhofii*, *Trygon walga*, and *Trygon bleekeri*, the large wide-open spiracles. In *Pteroplatœa* we know that the trophonemata pass into the spiracles; but the singular distension of these orifices in the other species, in contrast to the smallness of the other apertures of the body, points to the conclusion stated.

4. The stomach in all cases that we have hitherto observed (except in *Trygon bleekeri*, where the observation was lost) is empty, small, and displaced; while the colon (spiral gut) is full, large, and distended at the expense of other organs. And this leads us to the conclusion that the fœtal stomach is simply a channel through which the easily assimilable food passes to be absorbed by the spiral gut. And of this conclusion the presence of the unchanged "milk" in the spiral gut of *Myliobatis* is corroborative.

5. Finally, as to the method of respiration of the fœtus, no conclusion can as yet be arrived at. It is probably safe to assume that the consumption of oxygen and the production of carbonic acid by the fœtus are comparatively small, and that the respiratory exchanges are sufficiently carried out through the soft fœtal skin where this comes in contact with the vascular trophonemata and uterine wall.

Hitherto, we have not found any special distribution of blood-vessels to the skin, in the fœtus. And in *Pteroplatœa* the manner in which, when two fœtuses are present, the one is rolled up within the other, prevents contact of the inner fœtus with the uterine wall, except at the snout, and where the trophonemata enter the spiracles.

We are indebted to Professor G. B. Howes for calling our attention to a short note by Dr. W. A. Haswell ('Proceedings Linnean Society, New South Wales,' vol. 3, 1889, pp. 1713 to 1716) on *Urolophus*, in which it is suggested that the extraordinarily long external gills of the fœtus are concerned in absorbing matter which is supposed to exude from the blood-vessels of the uterine villi.

IV. "On some of the Variations observed in the Rabbit's Liver under certain Physiological and Pathological Circumstances." By T. LAUDER BRUNTON, M.D., B.Sc., F.R.S., and SHERIDAN DELÉPINE, M.B., B.Sc. Received October 22, 1891.

(Abstract.)

Under the influence of the natural stimulus of digestion, numerous changes are observable in liver cells. In this we partly confirm and partly complete (and add to) the observations of previous investigators.

1. Some of them indicate the existence of a peculiar kinetic state, manifested by *irritability and contractility, giving rise to variations in the distribution of the mitoma of the cells.*

The alternate enlargement and shrinking of the nuclei seem to point in the same direction.

The correlation between these phenomena and certain alternations in the contractility of the sphincter pupillæ still further strengthens this view.

2. During digestion, in addition to the dynamic state just alluded to, glycogen accumulates in the cells, and gradually fills up all the meshes of the mitoma. This begins in the hepatic zone almost immediately after the beginning of a meal, attains its maximum from the third to the eighth hour, and gradually diminishes, till at the twelfth hour only a few granules are left in the *hepatic zone, which is thus the first and the last to be infiltrated with glycogen.*

Another evidence of chemical activity is given by the accumulation of a ferruginous pigment in the cells of the liver. This begins to be well marked five hours after a meal; it gradually increases, until at the twelfth hour it has attained its maximum,* after which it rapidly diminishes. It is to be noted that the *first effect of taking a meal is to cause a diminution of this iron-containing pigment* in the liver cells.

There is, therefore, evidence that the signs of activity of liver cells occur in the following order:—

(*a.*) *Alterations in the Size of the Meshes and in the Distribution of the Mitoma.*—This occurs very early, and continues till the eighth hour at least.

(*b.*) Accumulation of some products which have been separated from the food and absorbed, but yet not assimilated† (or utilised in the production of energy) (*glycogen*). This becomes marked also early after the taking of a meal, and attains its maximum between the third and eighth hours.

(*c.*) Accumulation of some products which have been separated in the cell as a result of its special functional activity, but as yet not expelled from it (*iron-containing pigment*). This attains its maximum at the twelfth hour.

It is evident that the first and the last of these signs are not necessarily under the dependence of the absorption of food. On the contrary, the second is apparently one of the results of absorption. It is, therefore, probable that, whilst the first and the third may be brought about by various stimulating agents, the second, being con-

* As demonstrated by microchemical reactions. (It is, however, possible that part of the iron compounds set free by the splitting up of hæmoglobin may not be revealed by this method.)

† This is said with reference to the body taken as a whole.

nected with the taking of food, is hardly to be expected to be among the results of stimulation of starving cells.

In studying the effects of drugs it will be useful to consider the relation of these various effects in order to understand the special mode of action of the agent employed.

By these observations we have also obtained indications of the ways in which cells can be placed in different states of activity, so that, by the administration of drugs at various times after a meal, we can study more accurately what accelerating, restraining, or otherwise modifying, influences the drug may have.

We have been driven to consider, incidentally, some other problems, such as the relations which exist between contractility and secretory activity, but such things cannot be considered fully in this communication.

V. "On the Electromotive Phenomena of the Mammalian Heart." By W. M. BAYLISS, B.A., B.Sc., and ERNEST H. STARLING, M.D., M.R.C.P., Joint Lecturer on Physiology at Guy's Hospital. (From the Physiological Laboratory, University College, London.) Communicated by E. A. SCHÄFER, F.R.S. Received October 23, 1891.

(Abstract.)

Methods of Research.—The heart being exposed, two points of its surface were connected by means of non-polarisable electrodes with the terminals of a capillary electrometer. An image of the meniscus was thrown on to a moving photographic plate, on which were also recorded the contractions of the ventricle, a time tracing (8 or 100 per second), and in many cases the time of stimulation (when artificial stimuli were used), or the period of excitation of the vagus (when it was desired to slow the heart). In nearly all experiments we used dogs.

We have also made experiments on the excised heart. In these, the heart, immediately after the chest was opened, was placed in a warm moist chamber. The wires of the electrodes and the tube going to the tambour recording the heart beats passed through holes in the sides of the chamber.

Results.—As to the wave of negativity in the ventricle, we find that in animals whose hearts are in as normal a condition as possible the variation is always diphasic, the negativity at the base preceding that at the apex. The result is the same whether the pericardium be intact or opened, or whatever points of the ventricular surface are led off.

The character and direction of this variation, however, is exceedingly sensitive to slight changes in the temperature of the various parts of the heart, so that in order to obtain a constant result in animals with opened chest it is necessary to use warmed air for artificial respiration.

The following experiment illustrates the sensitiveness of the direction of the electrical variation to changes in the temperature of the respired air:—

June 27, 1890.—Dog: Operation as already described; artificial respiration with warmed air. Base of right ventricle (anterior surface) to acid, apex of left ventricle to capillary.

Direction of variation—

(*a.*) Before opening pericardium: diphasic—base, apex.

(*b.*) After opening pericardium: diphasic—base, apex.

The hot water was now poured out of the vessel surrounding the spiral, and this was filled with ice. After five minutes another photograph was taken of the variation—

(*c.*) Triphasic—base, apex, base.

(*d.*) (After another five minutes.) Diphasic—apex base.

(*e.*) The ice was now replaced by hot water.

After ten minutes more, the variation was found to be once again diphasic, the base becoming negative first.

Cold air was then used again, with the same result as before.

The same reversal of the variation can be obtained in the tortoise's heart by warming the apex and cooling the base simultaneously.

If we may regard the electrometer tracings as reliable, that is to say, if the variation—apex, base—with cooled base is the real variation, and it is not really a triphasic one with a first phase too small to be read on an electrometer, the only conclusion we can draw from our experiments is that the excitatory wave in the ventricle is a different thing from the wave of negativity, and precedes it (since in the hearts with cooled base, although the ventricles were beating in normal sequence to the auricles, the negativity began at the apex before the base).

Possibly, in the ventricles, the excitatory state is not transmitted directly from one muscle cell to another, but by the intervention of the intermuscular network of nerves shown by Dogiel, Openchowski, and others, to be universally present in the ventricular walls.

Time Measurements.—Some point of the auricles or ventricles was stimulated three times a second by means of an induction shock. In this way an artificial rhythm is induced, the heart contracting to every stimulation. This is the only method by which it is possible to get time measurements in the mammalian heart, since we cannot put this organ into a prolonged standstill, as we can the frog's heart by means of the Stannius ligature.

The latent period of electrical response of auricular and ventricular muscle to direct electrical stimulation is so short that we could not measure it accurately with the means at our disposal. It is certainly less than 0·01″.

We have sought to measure the rapidity of propagation of the wave in the mammalian ventricle in the same way as Engelmann and Sanderson and Page estimated it in the frog's heart, namely measuring the time interval between the beginning and the culmination of the initial phase.

In the exposed heart of a dog, breathing warmed air, the rate is generally about 30 mm. in 0·01″, *i.e.*, about 3 metres per second. But the sensitiveness of the form and direction of the variation to slight changes in temperature of different parts of the heart surface must make us hesitate in taking these figures as the correct ones.

There is a long period of delay in the passage across the auriculo-ventricular groove. A mean of eight observations gave 0·15″ as the time elapsed after stimulation of the auricles before the development of negativity at the base of the ventricles. Nearly the whole of this time is taken up in the passage from auricles to ventricles, since it makes very little difference to the time interval, whether the stimuli be applied to an auricular appendage, or the auricles close to the auriculo-ventricular groove.

Lastly, we have obtained no evidence of the supposed tetanic nature of a cardiac contraction (Fredericq), all our results pointing conclusively to the contraction being a single wave, starting at the base and passing thence to the apex of the heart.

The conclusions arrived at by Sanderson and Page in their work on the heart of the frog and tortoise hold good also for the mammalian heart.

Appendix.

On the Electrical Variation of the Heart of Man and the Intact Dog.

We have also photographed the electrical change of the heart of man and of the dog, with chest unopened, and, in opposition to Waller, we find that the variation is of such a nature as to show negativity always commencing at the base. The greatest effect was obtained by leading off from the apex beat and the right hand, but we found the same character of variation from whatever points on the surface of the body we led off, *i.e.*, the electrode nearest the base became negative first.

The photographs show what the eye could not distinguish clearly, viz., that each beat is accompanied by a triphasic variation, consisting of 1st, a "spike" (basal negativity); 2nd, a more prolonged excursion in the opposite direction (apical negativity); and 3rd, a large and prolonged movement in the same direction as the "spike" (basal nega-

tivity). Hence we conclude that the *base becomes negative before the apex*, and that its negativity overlasts that of the apex.

We do not feel able as yet to explain the triphasic nature of the variation; it shows, however, that normally the excitatory state at the base lasts longer than at the apex.

Presents, November 26, 1891.

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November 30, 1891.

ANNIVERSARY MEETING.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Report of the Auditors of the Treasurer's Accounts, on the part of the Society, was presented, by which it appears that the total receipts on the General Account during the past year, including balances carried from the preceding year and repayment of a mortgage loan of £15,000, amount to £22,433 7*s.* 3*d.* on the General Account, and that the total receipts on account of Trust Funds, including balances carried from the preceding year, amount to £6,096 12*s.* 10*d.* The total expenditure for the same period, including investments, amounts to £21,391 9*s.* 9*d.* on the General Account, and £2,340 9*s.* 11*d.* on account of Trust Funds, leaving a balance on the General Account of £1,022 5*s.* 8*d.* at the bankers', and £19 11*s.* 10*d.* in the hands of the Treasurer, and, on account of Trust Funds, a balance at the bankers' of £3,756 2*s.* 11*d.*

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary then read the following Lists :—

Fellows deceased since the last Anniversary (Dec. 1, 1890).

On the Home List.

Balfour, Thomas Graham, M.D.
Brady, Henry Bowman, F.G.S.
Carpenter, Philip Herbert, D.Sc.
Casey, John, M.R.I.A.
Croll, James, LL.D.
Duncan, Peter Martin, F.G.S.
Granville, George Leveson Gower, Earl, K.G.
Hawkshaw, Sir John, M.I.C.E.
Hewett, Sir Prescott Gardner, Bart., F.R.C.S.
Jeffery, Henry Martyn, M.A.
Jones, Thomas Wharton, F.R.C.S.
Marshall, John, F.R.C.S.
Moseley, Henry Nottidge, M.A.
Smith, Rt. Hon. William Henry.
York, His Grace William Thomson, Archbishop of, D.D.

On the Foreign List.

Becquerel, Edmond.
Nägeli, Carl Wilhelm von.
Weber, Wilhelm Eduard.

Fellows elected since the last Anniversary.

Anderson, William.
Bower, Prof. Frederick Orpen, D.Sc.
Conroy, Sir John, Bart., M.A.
Cunningham, Prof. Daniel John, M.D.
Dawson, George Mercer, D.Sc.
Elliott, Edwin Bailey, M.A.
Frankland, Prof. Percy Faraday, B.Sc.
Gilchrist, Percy Carlyle.
Halliburton, William Dobinson, M.D.
Hannen, Right Hon. James, Lord, D.C.L.
Heaviside, Oliver.
Jackson, Right Hon. William Lawies.
Marr, John Edward, M.A.
Mond, Ludwig.
Shaw, William Napier, M.A.
Thompson, Professor Silvanus Phillips, D.Sc.
Tizard, Captain Thomas Henry, R.N.

On the Foreign List.

Agassiz, Alexander.
Gould, Benjamin Apthorp.
Strasburger, Eduard.
Tacchini, Pietro.

The President then addressed the Society as follows :—

Since the last Anniversary Meeting the Royal Society have lost fifteen of their Fellows and three Foreign Members.

James Croll, who died on the 15th of December, at the age of sixty-nine, presented in his life a rare case of inborn passion for

philosophy and science conquering all obstacles and attaining to the object of life-long devotion to scientific research and philosophical speculation. Dependent wholly on his own work for his support, he commenced earning a livelihood as a beginner in a merchant's office; and with his ability he might, no doubt, have earned promotion and become a successful merchant. But the superior attraction of philosophy prevailed, and he wrote a book on the 'The Philosophy of Theism,' which was published in a large octavo volume, I believe while he was still working in the merchant's office. After being one out of about seventy unsuccessful candidates for the post of Under-Keeper of the Hunterian Museum of the University of Glasgow, he was appointed in 1859 to the post of Janitor of Anderson's College, Glasgow. About this time the Geological Society of Glasgow was founded, and became the centre of an active company of geologists, who took up the study of the traces of the Glacial period, so striking and abundant in the West of Scotland. Croll and his successful competitor for the University post, John Young, both of them with characteristic ardour, threw themselves into the work of geology. Croll, according to his peculiar bent of mind, was drawn chiefly into the more speculative lines of geological inquiry, and in 1864 published his essay on 'The Physical Cause of the Changes of Climate during the Glacial Epoch,' which deservedly gained the careful consideration both of geologists and of astronomers. This speculation undoubtedly presented a *vera causa* for some of the changes of climate which have occurred in geological history, although we can scarcely consider it adequate to be so powerful and exclusive a factor as Croll endeavoured to make it. His vigorous dispute with Carpenter regarding oceanic circulation rightly enforced attention to the importance of wind as the prime mover of some of the great ocean currents, but did not overthrow Carpenter's very important views regarding the effects of heat, according to which differences of temperature in the water itself in different regions and at different depths have paramount efficacy in producing some of the great oceanic circulations. After serving for eight years as Janitor in Anderson's College, Glasgow, Croll was selected by Sir Archibald Geikie to take charge of the maps and correspondence of the Geological Survey in Edinburgh. But, according to rule, he must be examined, and the Civil Service examiners plucked him in arithmetic and English composition. On the strong urgency of Sir Roderick Murchison (who asked me, from my personal knowledge of Croll, to write a statement of my opinion regarding his qualifications), the Civil Service Commissioners, with a wisely liberal relaxation of their rules, accepted his great calculations regarding the eccentricity of the earth's orbit and the precession of the equinoxes during the last ten million years as sufficient evidence of his arithmetical capacity, and his book on

'The Philosophy of Theism' and numerous papers published in scientific journals as proof of his ability to write good English. He was, therefore, allowed to receive the appointment in the Geological Survey in Edinburgh, though he had failed to pass the qualifying examination. During the rest of his life he was thus kept in relation with the great practical work of the Geological Survey in Scotland, and was allowed time to devote himself to speculative study and writing in geological physics, astronomy, and philosophy. During the last year of his life he sent to press his last work, published a few weeks before his death, entitled 'The Philosophical Basis of Evolution.'

The bitter winter of 1891 severely tried the health of many distinguished men. During the first seventeen days of January the Royal Society lost four Fellows.

John Marshall was Professor of Anatomy to the Royal Academy, and, as representative of the Royal College of Surgeons, President of the General Medical Council. His contributions to surgical literature, though not numerous, were considered of high value by those able to judge. He died, on the 1st January, at the age of seventy-two.

Dr. Casey, Fellow of the Royal University of Ireland, distinguished as a mathematician, was corresponding member of several scientific societies, and author of historical and elementary works on various branches of mathematics. He was in vigorous health until a short time before his death, when he was seized with bronchitis. He died, on January 3rd, at the age of seventy.

Dr. Brady's scientific reputation was mainly connected with his researches on the Rhizopoda and other minute forms of Invertebrate life. On these he published many memoirs of great value, by which knowledge was largely advanced. He was a Fellow of the Royal Society, the Linnean Society, and the Geological Society; and corresponding member of several foreign scientific bodies. He died, on January 10th, at the age of fifty-six. He bequeathed to the Royal Society all his books and papers relating to the Protozoa, with an additional benefaction to which I shall refer later.

Dr. Graham Balfour was Surgeon-General to the Army and Honorary Physician to the Queen, and President of the Royal Statistical Society. He died, on January 17th, at the age of sixty-eight.

Dr. Peter Martin Duncan, Professor of Geology in King's College, was well known, not only as a geologist who devoted himself especially to the study of fossil Corals and Echinoderms, and added greatly to knowledge by his valuable published memoirs on that subject, but also as a popular exponent of geology and zoology and an author and editor of works extending through the whole range of natural history. One great result of his work was a popular 'Natural History,' in six quarto volumes, brought out between the years 1878 and 1883, written by able specialists, on a comprehensive plan under

his own direction, and containing many articles written by himself. He died, on May 29th, at the age of sixty-seven.

Sir John Hawkshaw was undoubtedly one of the greatest engineers of this century. At the age of twenty-one he was appointed to take charge of important mining works in Venezuela, where he remained for three years, chiefly occupied in improving the navigation of the River Aroa for flat-bottomed boats employed to carry away the produce of the St. Felipe copper mines. Repeated attacks of fever and ague compelled him to return to England, but not until after many of the English miners employed under him, chiefly picked men from Cornwall, and several of the medical attendants of the station, had died from the effects of the unhealthy climate. The house in which he lived at the mines still exists, and bears his name. Soon after he left, all its inhabitants were murdered; and it has remained uninhabited ever since. From his return to England in 1834 until a few years before his death Hawkshaw was successfully occupied in the design, and in superintendence of the execution, of great engineering works; and in advising the Government, municipal corporations, and other public bodies, upon every variety of engineering questions. He early made his mark in engineering science and practice in respect to two important questions. In 1838 he reported to the Great Western Railway Company strongly against maintaining their broad gauge, and advocated a uniform gauge throughout the country; a few years later, in a keen contest of opinion against Robert Stephenson, he urged the practicability and advantageousness of introducing steeper gradients. The soundness of his views on both these questions is now generally admitted: and the introduction of steeper gradients, in consequence of his advocacy, led to a rapid extension of railways in all parts of the world. He was President of the Institution of Civil Engineers, 1862–63, and President of the British Association at Bristol in 1875. He died, on June 2nd, at the age of eighty.

Sir Prescott Gardner Hewett was Professor of Human Anatomy and Surgery in the College of Surgeons; and became President of the College as successor to Sir James Paget in 1883. He won high reputation, also, as an artist. Even while most occupied in his arduous profession as a surgeon, he took recreation in painting and drawing with a persevering zeal and a high degree of success rare among amateurs. He died on the 19th of June, at the age of seventy-nine.

Dr. Philip Herbert Carpenter was a member of the scientific staff of the deep-sea exploring expeditions of Her Majesty's steamships "Lightning" (1868) and "Porcupine" (1869–70); and in 1875 he was appointed Assistant Naturalist to Her Majesty's ship "Valorous," which accompanied Sir G. Nares' Arctic expedition to Disco

Island, and spent the summer sounding and dredging in Davis Strait and the North Atlantic. He devoted himself continuously, from 1875, to studying the morphology of the Echinoderms, more particularly of the Crinoids, both recent and fossil. He wrote numerous papers, which were published in the Transactions of the Royal Society and of the Linnean and Geological Societies. In 1883 he was awarded the Lyell Fund by the Geological Society of London, in recognition of the value of his work, and in 1885 was elected a Fellow of the Royal Society. In 1877 he was appointed Assistant-Master at Eton, especially charged with the teaching of biology, and held this post till his death, on the 21st October, at the age of thirty-nine.

Dr. Henry Moseley, Linacre Professor of Human and Comparative Anatomy in the University of Oxford, one of the eminent naturalists of the "Challenger" expedition, who served on board the "Challenger" during the entire voyage round the world, from 1872 till 1876, died on the 10th of this month, at the age of forty-six. He was author of many important papers in various branches of natural history, chiefly comparative anatomy and marine zoology.

Henry Martyn Jeffery, after taking high places in the Mathematical and Classical Triposes at Cambridge in 1849, commenced professional life as Lecturer in the College of Civil Engineers in Putney; and in later years continued it as Headmaster of Pate's Grammar School, Cheltenham, until he retired in 1882. As a teacher he was largely occupied with classics, but his favourite study was mathematics, and he is well known as the author of a long and continuous series of papers on subjects of pure mathematics which have been published in the 'Quarterly Journal of Mathematics,' the 'Journal of the London Mathematical Society,' and the 'Reports of the British Association.' He was actively occupied to the last with mathematical work and in the preparation of a text-book on his favourite mathematical subjects. He died, on the 3rd of November, at the age of sixty-six.

Thomas Wharton Jones, a distinguished physiologist, died on the 7th of this month, at nearly eighty years of age. Professor Huxley was one of his pupils forty years ago and gives bright and pleasant reminiscences of intercourse with his "old master."

Three distinguished men occupying high positions in the State, Fellows of the Royal Society, his Grace William Thomson, D.D., Lord Archbishop of York; the Right Honourable George Leveson-Gower, K.G., Earl of Granville; and the Right Honourable William Henry Smith, M.P.; died during the past year at the ages of seventy-two, seventy-six, and sixty-six.

The career of Carl Wilhelm von Nägeli, of Munich, during fifty years of most active and fertile scientific work, is of special interest in the history of botany and of biological speculation. He was elected

Foreign Member of the Royal Society in 1881, and died on the 10th of May, 1891, at the age of seventy-four.

The name of Becquerel has been famous in science since the days of Biot, Davy, De La Rive, Faraday, Ampère, and Arago. I well remember going to the Jardin des Plantes, in Paris, in January 1845, with an introduction from Professor James Forbes to Antoine César Becquerel, who, even at that remote time, was a veteran in physical science; and finding him in his laboratory there, assisted in work regarding electrolytically deposited films on polished metallic surfaces and their colours, by his son Edmond, a bright young man who had already commenced following his father's example as an active worker in experimental physics. He had been associated in 1839 with his father and the still older veteran, Biot, in experiments on phosphorescence produced by electric currents, a subject the profound importance of which is more appreciated now than it was then. Through fifty years of active and fruitful work in many departments of physical science, that subject of phosphorescence remained a speciality with Edmond Becquerel; and his son Henri, who survives him, has, in his turn, taken it up and given important contributions to knowledge regarding it. Edmond Becquerel was elected Foreign Member of the Royal Society in 1888, and died on the 11th of May, 1891, at the age of seventy-one.

Wilhelm Eduard Weber, of Göttingen, the second of three sons of Michael Weber (Professor of Positive Divinity at the beginning of this century in Wittenberg), of whom two were Foreign Members of the Royal Society and all three active workers for the advancement of natural knowledge, was elected Foreign Member of the Royal Society in 1850, and died on the 24th of June, 1891, at the age of eighty-seven. He was colleague of Gauss in the great work on magnetic measurement and on terrestrial magnetism of which they gave fruits to the world in the 'Resultate aus den Beobachtungen des Magnetischen Vereins.' The system of absolute measurement which Gauss introduced for magnetism in general, and applied practically to terrestrial magnetism, was nobly followed up by Weber, in extending it to electromagnetism and electrostatics, a truly epoch-making work in physical science. On it is founded the splendidly valuable system of practical measurement, in absolute units, of electric resistance, of electromotive-force, and of electric current, which, after a first introduction into this country in the year 1851, and a forty years' struggle, has, since the last Anniversary Meeting of the Royal Society, become definitively legalised for England through the action of the Board of Trade, advised by a Committee to which the Royal Society, the British Association, and the Institution of Electrical Engineers were invited to send, and sent, representatives.

The Royal Society, since the last Anniversary Meeting, have been,

as always, active both in the proceedings of their ordinary meetings, which have been full of scientific interest, and in the conduct of the important affairs committed to their Council. During the past year nineteen memoirs have been published in the 'Philosophical Transactions,' containing a total of 1020 pages and 60 plates. Of the 'Proceedings,' six numbers have been issued, containing 893 pages. Of the large number of papers which have been published in the 'Proceedings' two-thirds are on the physics and dynamics of dead matter and one-third on biological subjects.

As stated by Sir George Stokes in his Presidential Address at the last Anniversary Meeting, a revision of the whole body of the Statutes of the Royal Society had been entered upon, a Committee had recently reported to the Council, and their report had been left to the new Council then entering on office to take such action in the matter as might be judged proper. The Council now concluding their term of office have accordingly given much time to the subject, and have completed the work of re-enacting the Statutes with such amendments as have seemed desirable. The only questions upon which there was effective difference of opinion were those connected with the election of Fellows, which were referred to by Sir George Stokes as having elicited considerable difference of opinion in the reporting Committee. The Council, after much anxious consideration, resolved to make no change of the existing Statutes in this respect.

There have been no changes during the past session in the constitution of the staff employed in the Offices and Library; but in the Catalogue Department, two lady assistants and two copyists have been engaged to work under the superintendence of Miss Chambers, who succeeded in July of last year to the post rendered vacant by the death of the late Mr. Holt, and who continues to give every satisfaction in the discharge of her duties.

In January of the present year a communication was received from our Fellow Professor G. S. Brady, intimating that his brother, the late Mr. Henry Bowman Brady, whose decease I have already mentioned, had bequeathed to the Society all his books and papers relating to the Protozoa, with the recommendation that they should be kept together as a distinct collection. In case this recommendation should be adopted, a further bequest of £300 was made, the interest of principal or both to be applied, at the discretion of the Council, to the purchase of works on the same or kindred subjects, to be added to the collection. The Council have accepted both these bequests, and a case marked with an engraved plate has been set aside in the Library for the accommodation of the Brady collection.

His Excellency Robert Halliday Gunning, M.D., LL.D., F.R.S.E., who in 1887 founded certain scholarships and prizes, called the Victoria Jubilee Prizes, for the promotion of original scientific work

and proficiency in scientific education in connection with the Royal Society of Edinburgh, the University of Edinburgh, and other institutions in that city, desires to institute foundations of a similar kind in London. He has accordingly given to the Royal Society a sum of £1000, to be ultimately invested in such manner as the President and Council, in their absolute and uncontrolled discretion, may think fit, and to be held in trust always for the purpose of forming a fund the annual income of which shall be applied triennially towards the promotion of physical science and biology in such manner as to the President and Council of the Royal Society may appear most desirable. The President and Council, for the time being, are given full power to make such rules and regulations as they think fit with regard to the application of the income of the fund, which "shall always be kept distinct from and not in any way immixed with the general funds of the Royal Society."

A very important resolution for the advancement of natural knowledge has been adopted during the past year by the Royal Commissioners of the Exhibition of 1851, in the institution of the Exhibition Science Scholarships, to which, after the first year, an expenditure to the extent of £5,000 a year is to be devoted. Sixteen appointments have already been made to scholarships of £150, to be held for two years, with possible renewal for a third year. The Commissioners require of each candidate for an appointment satisfactory evidence of proficiency in a three years' course of University or high class College study, and of capacity for experimental work. To the tenure of each scholarship the duty is assigned of advancing science by experimental work in physics, mechanics, chemistry, or any application of science tending to benefit our national industries.

A Committee of the British Association appointed for the purpose of reporting on the best means of comparing and reducing observations on terrestrial magnetism has strongly recommended the re-establishment of a magnetic observatory at the Cape of Good Hope. A conference on the subject was held between the Committee and Dr. Gill, the Astronomer Royal of the Cape of Good Hope, last June, during his recent visit to England, which has resulted in an application to the Admiralty to carry this recommendation into practical effect in connection with the astronomical observatory of the Cape of Good Hope (belonging to the Admiralty). This application is at present under the consideration of the Admiralty.

A fundamental investigation in astronomy, of great importance in respect to the primary observational work of astronomical observatories, and of exceeding interest in connection with tidal, meteorological, and geological observations and speculations, has been definitively entered upon during the past year, and has already given substantial results of a most promising character. The International

Geodetic Union, at its last meeting in the autumn of 1890, on the motion of Professor Foerster, of Berlin, resolved to send an astronomical expedition to Honolulu, which is within 9° of the opposite meridian to Berlin (171° west from Berlin), for the purpose of making a twelve months' series of observations on latitude corresponding to twelve months' analogous observations to be made in the Royal Observatory, Berlin. Accordingly Dr. Marcuse went from Berlin, and, along with Mr. Preston sent by the Coast and Geodetic Survey Department of the United States, began making latitude observations in Honolulu about the beginning of June. In a letter from Professor Foerster, received a few weeks ago, he tells me that he has already received from Honolulu a first instalment of several hundred determinations of latitude, made during a first three months of the proposed year of observations; and that, in comparing these results with the corresponding results of the Berlin Observatory, he finds beyond doubt that in these three months the latitude increased in Berlin by one-third of a second and decreased in Honolulu by almost exactly the same amount. Thus, we have decisive demonstration that motion, relatively to the Earth, of the Earth's instantaneous axis of rotation, is the cause of variations of latitude which had been observed in Berlin, Greenwich, and other great observatories, and which could not be wholly attributed to errors of observation. This, Professor Foerster remarks, gives observational proof of a dynamical conclusion contained in my Presidential Address to Section A of the British Association, at Glasgow, in 1876, to the effect that irregular movements of the Earth's axis to the extent of half a second may be produced by the temporary changes of sea-level due to meteorological causes.

It is proposed that four permanent stations for regular and continued observation of latitude, at places of approximately equal latitude and on meridians approximately 90° apart, should be established under the auspices of the International Geodetic Union. The reason for this is that a change in the instantaneous axis of rotation in the direction perpendicular to the meridian of any one place would not alter its latitude, but would alter the latitude of a place 90° from it in longitude by an amount equal to the angular change of the position of the axis. Thus two stations in meridians differing by 90° would theoretically suffice, by observations of latitude, to determine the changes in the position of the instantaneous axis; but differential results, such as those already obtained between Berlin and Honolulu, differing by approximately 180° in longitude, are necessary for eliminating errors of observation sufficiently to give satisfactory and useful results. It is to be hoped that England, and all other great nations in which science is cultivated, will co-operate with the International Geodetic Union in this important work.

Among the most interesting scientific events of the past year was the celebration of the 100th anniversary of the birth of Faraday by the two Faraday Lectures in the Royal Institution last June. In the first of these, which was delivered by Lord Rayleigh, under the presidency of the Prince of Wales, an old pupil of Faraday's and now Vice-Patron of the Royal Institution, a general survey of Faraday's work during his fifty-four years' connection with the Royal Institution was given. Naturally, a large part of the lecture was devoted to magnetism and electricity and to electro-magnetic induction; but it contained also much that must have been surprising to the audience, scarcely prepared to be told, as they were told by Lord Rayleigh, that "Faraday's mind was essentially mathematical in its qualities," and that, particularly in his acoustical work, he had made many very acute observations of physical phenomena, of a kind to help in guiding the mathematician to the solution of difficult and highly interesting problems of mathematical dynamics, and in some cases actually to give him the solution surprisingly different from what might have been expected even by highly qualified mathematical investigators.

The other Faraday Lecture, given by Professor Dewar, was a splendid realisation of Faraday's anticipations regarding the liquefaction of the "permanent gases," according to which no extreme of pressure might be capable of liquefying hydrogen or oxygen at ordinary temperature, while a very moderate pressure might suffice to liquefy them if their temperatures could be sufficiently lowered. Professor Dewar actually showed liquid oxygen in a glass tumbler, not boiling or in a state of commotion like a tumbler of soda-water, but quietly and without any sensible motion keeping itself cool by its own evaporation, while it rapidly formed a thick jacket of hoar-frost on the outside of the vessel by condensation of watery vapour from the surrounding atmosphere. The surprise and delight of the audience reached a climax when liquid oxygen was poured from one open vessel to another before their eyes.

A matter of great importance in respect to the health of the community was submitted to the Royal Society by the London County Council, in a letter of date May 1, 1891, asking for information and suggesting investigation regarding the vitality of microscopic pathogenic organisms in large bodies of water, such as rivers which are sources of water-supply and which are exposed to contamination. After some correspondence it was agreed, between the County Council and the Council of the Royal Society, to enter upon an investigation, the expense of which was to be defrayed partly by the London County Council and partly by the Royal Society out of the Government Grant for Scientific Research. When we consider how much of disease and death is due to contaminated water, we must feel that it is scarcely possible to overestimate the vital importance of the pro-

posed investigation. Let us hope that the alliance between the London County Council and the Royal Society, for this great work, may be successful in bringing out practically useful results.

The President then presented the Medals awarded by the Society, as follows :—

Professor Stanislao Cannizzaro (Copley Medal).

Stanislao Cannizzaro, Senator of Italy, and Professor of Chemistry in the University of Rome, has rendered invaluable service to the philosophy of modern chemical science. The work of Avogadro, in 1811, and afterwards that of Ampère, had already thrown much light on the relative weights of the molecules of elementary bodies, and on the proportions in which those weights enter into chemical combination. But it is to Cannizzaro that we owe the completion of what they had left unfinished. He pointed out the all-important difference, hitherto overlooked, between molecular and atomic weights, and showed—(1) How the atomic weights of the elements contained in a volatile compound can be deduced from the molecular weights of such compounds; (2) how the atomic weights of the elements the vapour-densities of whose compounds were unknown can be ascertained by help of their specific heats. By these investigations the series of atomic weights of the elements, the most important of all chemical constants, and the relation which these weights bear to the molecular weights of the elements, have been placed on the firm basis on which they have ever since rested. It is to Cannizzaro that science is indebted for this fundamental discovery, and it is this which it is proposed to recognise by the award of the Copley Medal.

Professor Charles Lapworth, F.R.S. (Royal Medal).

Professor Lapworth is the author of some of the most original and suggestive papers which have appeared in the geological literature of this country for the last twenty years. Special reference may be made to his researches on graptolites, and to his patient investigation by these means of the exceedingly complicated structure of the Silurian uplands of the South of Scotland. He has been able not only to supply the key which has given the solution of the stratigraphical difficulties of that region, but also to furnish theoretical geology with an array of new facts from which to philosophise as to the mechanism of mountain-making. Of not less importance are his detailed studies of the structure of the North-west Highlands and his demonstration of the true order of stratigraphical sequence in that region of complex disturbance. As a stratigraphist he has attained the highest rank, and he has likewise made himself a chief palæonto-

logical authority on the structure and distribution of the Graptolitidæ. For some years past he has been engaged in a laborious study of the Silurian and Cambrian rocks of the middle of England, the detailed publication of which is awaited with much interest by geologists.

Professor Rücker, F.R.S. (Royal Medal).

In conjunction with Professor Reinold, Professor Rücker carried out an important series of researches (extending over ten years) on the electric resistance and other physical properties of liquid films, in the course of which the fact was established that the black part of a soap film in equilibrium has a uniform or nearly uniform thickness of 11 or 12 micromillimetres, and that there is an abrupt augmentation across its border to a thickness of about 30 or 40 micromillimetres in passing to the coloured portions. This, considered in connection with the well-known sudden opening out of the little black areas in an ordinary soap-bubble, proves a minimum of surface-tension for some thickness between 10 and 50 micromillimetres, which, in the ordinary soap-bubble unmodified by Reinold and Rücker's electric current, is temporarily balanced in virtue of the abrupt change of thickness, a proposition of fundamental importance in the molecular theory, implying the existence of molecular heterogeneousness.

In theoretical calculations connected with the compounding of dynamos and motors to produce constant potential difference, constant current, or constant speed, electricians did not see their way to obtain results of a sufficiently simple character to be of use in practice, if they employed a function of the current which fairly represented the magnetism. They were, therefore, compelled to assume in such calculations that the magnetism was a linear function of the current, although it was well known that this was very far from being true when the current was large. Professor Rücker, however, developed a simple method of attacking such problems, and showed how the magnetic saturation of the iron might be taken into account, and a comprehensive solution of the general problem of compounding dynamos and motors obtained in a workable form. Professor Rücker's paper containing his investigation, and which will be found in the 'Proceedings of the Physical Society,' is a most valuable contribution to the theory of direct-current dynamos and motors.

Professor Rücker has, with the co-operation of Professor Thorpe, completed a Magnetic Survey of the British Isles (1884–89), which, independently of its great value in investigations of the distribution of the earth's magnetism, and the changes to which it is subject, is specially remarkable for the exhaustive discussion of the observations in reference to regions of local magnetic disturbance, and their relation to the geological constitution of the earth's crust in the neigh-

bourhood. Professor Rücker has followed up this discussion by a paper on "The Relation between the Magnetic Permeability of Rocks and Regional Magnetic Disturbances," read before the Royal Society. The high estimate that has been formed of the value of this Magnetic Survey is perhaps most easily appreciated from the very large sums that the Government Grant Committee have recommended should be contributed to aid in the completion of this work of international importance.

Professor Victor Meyer (Davy Medal).

Professor Victor Meyer, formerly the successor of Wöhler at Göttingen, and who now occupies the chair of Bunsen at Heidelberg, is eminent as an original worker and discoverer in almost every branch of chemical science. His methods of determining the vapour densities of substances have been of the greatest service to chemists, not only as convenient and generally applicable modes of ascertaining atomic and molecular weights, but also as serving to throw light on the molecular constitution of elements and compounds under varying conditions of temperature and pressure. A striking example of the value of these methods is seen in their application by their author to the study of the molecular dissociation of the element iodine—one of the most masterly investigations of recent years, and which is universally recognised as of the very highest significance and importance. Not less noteworthy are Victor Meyer's services to organic chemistry. His work on the nitroso-bodies, and his brilliant discovery of thiophene, the initial member of a class of substances hitherto unknown, his subsequent synthetical formation of it, and the remarkable series of researches on its derivatives, in part carried out with the aid of his pupils, stamp him as an investigator of exceptional power and distinction.

The Statutes relating to the election of Council and Officers were then read, and Mr. Crookes and Prof. Meldola having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—Sir William Thomson, D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor Michael Foster, M.A., M.D.
The Lord Rayleigh, M.A., D.C.L.

Foreign Secretary.—Sir Archibald Geikie, LL.D.

Other Members of the Council.

Captain William de Wiveleslie Abney, C.B.; William Thomas Blanford, F.G.S.; Professor Alexander Crum Brown, D.Sc.; Professor George Carey Foster, B.A.; James Whitbread Lee Glaisher, D.Sc.; Frederick Ducane Godman, F.L.S.; John Hopkinson, D.Sc.; Professor George Downing Liveing, M.A.; Professor Joseph Norman Lockyer, F.R.A.S.; Professor Arthur Milnes Marshall, D.Sc.; Philip Henry Pye-Smith, M.D.; William Chandler Roberts-Austen, F.C.S.; Professor Edward Albert Schäfer, M.R.C.S.; Sir George Gabriel Stokes, Bart, M.A.; Professor Sydney Howard Vines, M.A.; General James Thomas Walker, C.B.

The thanks of the Society were given to the Scrutators.

Balance Sheet. 1891.

Statement of Receipts and Expenditure from November 12th, 1890, *to November* 12th, 1891.

Receipts	£ s. d.	£	s.	d.
To Balance at Bank, 12th November, 1890		1,753	9	5
,, Balance in hand, Catalogue Account	2 15 1	25	18	6
,, ,, Petty Cash	23 3 5			
,, Compositions		240	0	0
,, Admission Fees		20	0	0
,, Annual Contributions, 142 at £4	£568 0 0	1,057	0	6
,, ,, 163 at £3	489 0 0			
,, Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions		312	0	0
,, Rents:				
Fee Farm, Lewes	18 14 5	116	4	5
Mablethorpe Estate	97 10 0			
,, Ground Rents		604	10	6
,, Dividends (exclusive of Trust Funds)		1,999	8	5
,, Interest on Mortgage Loan:				
W. H. Long	356 12 1	411	4	1
Duke of Norfolk	54 12 0			
,, Sale of Transactions and Proceedings		565	15	2
,, Sale of Catalogue		42	18	9
,, Sale of Krakatoa Report (leaving £91 12*s.* 3*d.* Expenditure in excess of Receipts)		3	5	0
,, Transfer from Handley Fund on account of Catalogue		187	2	4
,, Lendenfeld Monograph, Sales (leaving £678 6*s.* 9*d.* expenditure in excess of receipts)		4	0	0
,, Mortgage Loan, repaid		15,000	0	0
,, Interest on Bank Deposit Account		90	10	2
		£22,433	7	3

Expenditure	£ s. d.	£	s.	d.
By Salaries, Wages, and Pension		1,662	13	6
,, Catalogue of Scientific Papers		323	7	6
,, Books for the Library		382	11	10
,, Printing and Advertising Transactions, and Separate Copies to Authors and Publisher	607 9 11	2,446	12	6
,, Ditto Proceedings, Nos. 295 to 302	460 4 3			
,, Ditto Miscellaneous	115 3 9			
,, Paper for Transactions and Proceedings	315 12 0			
,, Binding ditto	44 9 0			
,, Engraving and Lithography	903 13 7			
,, Soirée and Reception Expenses		162	5	6
,, Coal, Lighting, &c.	220 14 11	868	6	1
,, Office Expenses	47 7 7			
,, House Expenses (including painting Library)	369 9 3			
,, Tea Expenses	18 7 5			
,, Fire Insurance	55 5 0			
,, Taxes	45 7 6			
,, Advertising	20 4 6			
,, Postage, Parcels, and Petty Charges	64 4 9			
,, Miscellaneous Expenses	27 5 2			
,, Law Charges		15	12	10
,, Carrington Donation		30	0	0
,, Challenger Report, payment to Mr. J. Murray (Leaving balance in hand of £500.)		500	0	0
,, £4,000 Southern Mahratta Railway 4 per Cent. Debenture Stock at $117\frac{3}{4}$	4,745 13 6	15,000	0	0
,, £2,000 India $3\frac{1}{2}$ per Cent. at $106\frac{3}{8}$	2,135 4 0			
,, £5,185 0*s.* 3*d.* Consolidated $2\frac{3}{4}$ per Cent. Stock at $94\frac{3}{4}$	4,919 2 6			
,, Mortgage Loan to Duke of Norfolk	3,200 0 0			
,, Balance at Bankers		1,022	5	8
,, Balance on hand, Catalogue Account	£4 14 1	19	11	10
,, Ditto, Petty Cash	14 17 9			
		£22,433	7	3

Trust Funds.

	£	s.	d.	£	s.	d.
To Balance at Bank 12th November, 1890:—						
General Account	1,579	2	5			
Fee Reduction Fund Account	93	12	1			
Scientific Relief Fund Account	732	5	6	2,559	3	11
Joule Memorial Fund	154	3	11			
,, Scientific Relief Fund, Dividends, and Donations	1,159	5	11			
,, Donation Fund, Dividends, and Transfer from Jodrell Fund	389	15	6			
,, Rumford Fund, Dividends	62	5	4			
,, Bakerian and Copley Medal Fund, Dividends, &c.	54	10	0			
,, Keck Bequest, Dividends	23	8	0			
,, Wintringham Fund, Dividends, &c.	32	4	0			
,, Croonian Lecture Fund—Rent	100	19	4			
,, Davy Medal Fund, Dividends	32	3	6	3,537	8	11
,, Gassiot Trust, Dividends	498	4	8			
,, Handley Fund, Dividends	187	2	4			
,, Jodrell Fund, Dividends	139	3	4			
,, Fee Reduction Fund, Dividends	423	3	0			
,, Darwin Memorial Fund, Dividends	85	16	0			
,, Joule Memorial Fund, Subscriptions and Dividends	49	8	0			
,, Brady Library Account	300	0	0			
				£6,096	12	10

	£	s.	d.	£	s.	d.
By Scientific Relief Fund, Grants	441	0	0			
,, Donation Fund, Grants	302	13	0			
,, Rumford Fund, Medal	59	4	11			
,, Bakerian and Copley Medal Fund, Medal, and Gift	62	12	0			
,, Keck Bequest, Payment to Foreign Secretary	23	8	0			
,, Wintringham Fund, Payment to Foundling Hospital	32	4	0			
,, Croonian Lecture Fund, Payment	100	19	4			
,, Davy Medal Fund, Gold Medals	34	10	6	2,340	9	11
,, Gassiot Trust, Payments to Kew Committee	487	10	0			
,, Handley Fund, Transfer to General Account, for Catalogue	187	2	4			
,, Jodrell Fund, Transfer to Donation Fund	139	3	4			
,, Fee Reduction Fund, Transfer to Royal Society General Account (1891)	312	0	0			
,, Darwin Memorial Fund, Medal and Gift	158	2	6			
,, Balance at Bankers:—						
General Account	1,897	4	6			
Fee Reduction Fund Account	204	15	1	3,756	2	11
Scientific Relief Fund Account	1,450	11	5			
Joule Memorial Fund Account	203	11	11			
				£6,096	12	10

Estates and Property of the Royal Society, including Trust Funds.

Estate at Mablethorpe, Lincolnshire (55A. 2R. 2P.), rent £100 per annum.

Ground Rent of House, No. 57, Basinghall Street, rent £380 per annum.

" " of 23 houses in Wharton Road, West Kensington, rents £253 per annum.

Fee Farm Rent, near Lewes, Sussex, £19 4*s*. per annum.

One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, about £52 per annum, Croonian Lecture Fund.

Stevenson Bequest. Chancery Dividend. One-fourth annual interest on Bank Stock and other Securities (produced £609 15*s*. 11*d*. in 1890-91).

The Funds in Court now standing to the credit of the cause are as follows:—

£11,000 Bank Stock.
£11,031 London and North Western Railway Consolidated 4 per Cent. Guaranteed Stock.
£11,105 Great Northern Railway 4 per Cent. Perpetual Preference Stock.
£11,031 North Eastern Railway Consolidated 4 per Cent. Guaranteed Stock.
£8,894 Great Western Railway 5 per Cent. Consolidated Guaranteed Stock.
£11,035 16*s*. 5*d*. Midland Railway 4 per Cent. Consolidated Preference Stock.

Subject to certain charges, the Royal Society is entitled to one-fourth of the proceeds.

£3,200 Mortgage Loan, $3\frac{1}{2}$ per Cent.

£19,482 8*s*. 8*d*., $2\frac{3}{4}$ per Cent. Consolidated Stock, being £10,779 8*s*. 2*d*. on account of the following Funds:—

	£	*s*.	*d*.
Rumford Fund	2,330	0	0
Wintringham Fund	1,200	0	0
Gassiot Trust	400	0	0
Sir J. Copley Fund	1,666	13	4
Jodrell Fund	5,182	14	10

£5,185 0*s*. 3*d*. General Purposes.
and £3,518 0*s*. 3*d*. in Chancery, arising from sale of the Coleman Street Estate.—General Purposes.

£403 9*s*. 8*d*. New $2\frac{1}{2}$ per Cent. Stock.—Bakerian and Copley Medal Fund.

£3,000 India $3\frac{1}{2}$ per Cent. Stock.—General Purposes.

£800 Midland Railway 3 per Cent. Debenture Stock.—Keck Bequest.

£5,660 Madras Railway Guaranteed 5 per Cent. Stock { General Purposes, £5,000. Davy Medal Fund, £660. }

£10,000 Italian Irrigation Bonds.—The Gassiot Trust.

£8,528 Great Northern Railway 3 per Cent. Debenture Stock { Scientific Relief Fund, £6,666 13*s.* 4*d.* / The Trevelyan Bequest, £1,861 6*s.* 8*d.*

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.—Donation Fund.

£4,400 Metropolitan $3\frac{1}{2}$ per Cent. Stock.—Fee Reduction Fund.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.—Fee Reduction Fund.

£18,150 " " " " 4 per Cent. Consolidated Guaranteed Stock.—{ £6,000 Scientific Relief Fund. / £12,150 General Purposes.

£5,000 London and North Western Railway Consolidated 4 per Cent. Preference Stock.—General Purposes.

£5,000 North Eastern Railway 4 per Cent. Preference Stock.—General Purposes.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.—Darwin Memorial Fund.

£4,340 South Eastern Railway 5 per Cent. Debenture Stock.—Scientific Relief Fund.

£3,333 London and South Western Railway 4 per Cent. Preference Stock.—General Purposes.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.—Handley Fund.

£800 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.—Joule Memorial Fund.

£4,000 Southern Mahratta Railway 4 per Cent. Debenture Stock.—General Purposes.

£300 on Deposit Account at Bank.—Brady Library Account.

£300 on Deposit Account on behalf of the Committee.—Joule Memorial Fund.

£1,000 Policy in the Atlas Assurance Office, becoming due October 7th, 1899.—Catalogue Account.

£1,000 Bond.—Dr. Gunning.—Interest to be applied to the promotion of Physics and Biology.

JOHN EVANS, *Treasurer.*

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct.

M. FOSTER.
HUGO MÜLLER.
J. T. WALKER.

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct.

JAMES COCKLE.
FRANCIS GALTON.

Trust Funds. 1891.

Scientific Relief Fund.

£6,000 L. & N.W.R. 4 per Cent. Consolidated Guaranteed Stock.
£6,666 13*s*. 4*d*. Great Northern Railway 3 per Cent. Debenture Stock.
£4,340 South Eastern Railway 5 per Cent. Debenture Stock.

Dr.

	£	*s.*	*d.*	£	*s.*	*d.*
To Balance { Income	756	15	9			
Less — Capital over-invested	24	10	3			
				732	5	6
,, Dividends				640	11	6
,, Annual Subscriptions				5	0	0
,, Interest on Deposit				13	14	5
,, H. B. Brady. Bequest				500	0	0
				£1,891	11	5

Cr.

				£	*s.*	*d.*
By Grants				441	0	0
,, Balance, Income	£970	1	8	1,450	11	5
,, Capital	480	9	9			
				£1,891	11	5

Donation Fund.

£5,030 Great Northern Railway Perpetual 4 per Cent. Guaranteed Stock.
The Trevelyan Bequest. £1,861 6*s*. 8*d*. Great Northern Railway 3 per Cent. Debenture Stock.

	£	*s.*	*d.*
To Balance	789	12	5
,, Dividends	250	12	2
,, Transfer from Jodrell Fund	139	3	4
	£1,179	7	11

	£	*s.*	*d.*
By Grants	302	13	0
,, Balance	876	14	11
	£1,179	7	11

Rumford Fund.

£2,330 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	142	18	1	By Medal	59	4	11
,, Dividends	62	5	4	,, Balance	145	18	6
	£205	3	5		£205	3	5

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* $2\frac{3}{4}$ per Cent. Consolidated Stock.
£403 9*s.* 8*d.* New $2\frac{1}{2}$ per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	119	2	6	By Gold Medal	4	12	0
,, Dividends, New $2\frac{1}{2}$ per Cent. Stock	9	16	8	,, Professor Newcomb, Sir J. Copley's Gift	50	0	0
,, Dividend—Sir J. Copley's Fund	44	13	4	,, ,, Schuster.... £4 } ,, G. H. Darwin........... 4 } Bakerian Lecture	8	0	0
				,, Balance	111	0	6
	£173	12	6		£173	12	6

The Keck Bequest.

£800 Midland Railway 3 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Dividends	23	8	0	By Payment to Foreign Secretary	23	8	0

Wintringham Fund.

£1,200 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	32	4	0	By Payment to Foundling Hospital	32	4	0
,, Dividends	32	4	0	,, Balance	32	4	0
	£64	8	0		£64	8	0

Croonian Lecture Fund.

One-fifth of the clear rent of an Estate at Lambeth Hill, from the College of Physicians, about £52 per annum.

	£	s.	d.		£	s.	d.
To Rent	100	19	4	By Lecture (1890)—Professor Marshall Ward	50	9	8
				,, (1891)—Professors Gotch and Horsley	50	9	8
	£100	19	4		£100	19	4

Davy Medal Fund.

£660 Madras Railway Guaranteed 5 per Cent. Stock.

	£	s.	d.		£	s.	d.
To Balance	76	19	1	By Gold Medals	34	10	6
,, Dividends	32	3	6	,, Balance	74	12	1
	£109	2	7		£109	2	7

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.

£400 $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Balance	52	1	10	By Payments to Kew Committee	487	10	0
„ Dividends	498	4	8	„ Balance	62	16	6
	£550	6	6		£550	6	6

Handley Fund.

£4,798 Lancashire and Yorkshire Railway 4 per Cent. Guaranteed Stock.

	£	s.	d.		£	s.	d.
To Dividends	187	2	4	By Transfer to Catalogue Account	187	2	4

The Jodrell Fund.

£5,182 14*s.* 10*d.* $2\frac{3}{4}$ per Cent. Consolidated Stock.

	£	s.	d.		£	s.	d.
To Dividends	139	3	4	By Transfer to Donation Fund	139	3	4

Fee Reduction Fund.

£4,400 Metropolitan $3\frac{1}{2}$ per Cent. Stock.

£7,000 London and North Western Railway 4 per Cent. Perpetual Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	93	12	1	By Transfer to Royal Society General Account	312	0	0
„ Dividends	423	3	0	„ Balance	204	15	1
	£516	15	1		£516	15	1

Darwin Memorial Fund.

£2,200 South Eastern Railway 4 per Cent. Debenture Stock.

	£	s.	d.		£	s.	d.
To Balance	366	4	6	By Medal and Gift	158	2	6
,, Dividends	85	16	0	,, Balance	293	18	0
	£452	0	6		£452	0	6

Joule Memorial Fund.

£800 London, Brighton, and South Coast Railway Consolidated Guaranteed 5 per Cent. Stock.
£300 on Deposit on behalf of the Committee.

	£	s.	d.		£	s.	d.
To Balance	154	3	11	By Balance	203	11	11
,, Subscriptions	2	3	10				
,, Interest on Deposit	8	4	2				
,, Dividend	39	0	0				
	£203	11	11		£203	11	11

Brady Library Fund.

£300 on Deposit Account at Bank.

	£	s.	d.		£	s.	d.
To Bequest	300	0	0	By Amount placed on Deposit at Bank	300	0	0

The following Table shows the progress and present state of the Society with respect to the number of Fellows :—

	Patron and Royal.	Foreign.	Com-pounders.	£4 yearly.	£3 yearly.	Total.
Dec. 1, 1890 ..	5	49	168	145	150	517
Since Elected ..	..	..	+ 4	+ 1	+ 12	+ 17
Since Deceased ..	..	− 3	− 6	− 8	− 1	− 18
Nov. 30, 1891 ..	5	46	166	138	161	516

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the advancement of Science (continued from Vol. XLVIII, p. 486).

1890—1891.

	£
Prof. Piazzi Smyth, for further Research in Spectroscopic Measurement of Ultra Definition and Extreme Separation....	70
E. H. Griffiths, for Apparatus required in a Re-determination of the Value of Joule's Equivalent........................	50
A. M. W. Downing, to determine the Orbit of the Minor Planet Flora..	20
Dr. Edridge Green, to ascertain quantitatively the Percentage of Loss of Light in cases of Colour-blindness due to a Shortened Spectrum..	20
H. L. Callendar, for the Manufacture of a Standard Platinum Thermometer, and the Comparison of the same with an Air Thermometer at High as well as Low Temperatures..........	80
Prof. J. N. Lockyer, for Observations (chiefly Long Exposure Photographs) of Nebulæ, and of Groups III and V...........	125
J. Joly, for Extension of his Research on the Specific Heats of Gases at Constant Volume to Higher Pressures	40
Prof. G. H. Darwin, to make an Abacus with Card Guide Plates for the Reduction of Tidal Observations	50
J. H. Gray, for a Determination of the Thermal Conductivity of Metals...	50
Carried forward	£505

	£
Brought forward	505
Town Gardening Committee of the Manchester Field-Naturalists' Society, and Scientific Committee of the Royal Horticultural Society, for an Analysis of the Air and Fog of Manchester and Salford, and for further Inquiry into the Composition of London Fog and its Effects on Cultivated Plants	50
Dr. T. Ewan, for Apparatus to aid in a Research on the Absorption Spectra of Copper Salts in Solution, and the Changes which they undergo on Dilution, Heating, &c.	25
G. Higgs, for the Production by Photography of a Map of the Normal Solar Spectrum, &c., from w.l. 3,000 to 10,000....	50
Prof. Tait, for a Research on the circumstances of Impact, especially its Duration.	30
G. S. Turpin, for Continuation of a Research on the Ignition of Explosive Gaseous Mixtures	50
Prof. J. V. Jones, for a further Determination of the Ohm by the Method of Lorenz	50
Dr. W. Huggins, for the Continuance of his Work on the Motions and Constitution of the Stars and Nebulæ	100
Prof. Rücker and Prof. Thorpe, towards the expense of a Magnetic Survey of the United Kingdom.	600
Dr. J. B. Tingle, for an Investigation of the Action of various Ethereal Salts on Camphor and other Ketones, with special reference to the Elucidation of the Constitution of Camphor	15
Dr. Dittmar, for a Re-determination of the Atomic Weights of Potassium, Sodium, and Lithium.	100
Prof. W. H. Perkin, Jun., for a Research on the Constitution of Camphoric, Camphoronic, and allied Acids	75
J. N. Collie, for a Research on the Constitutional Formulæ of (1) Dehydracetic Acid; (2) Meconic and Pyromeconic Acids	20
Dr. H. Marshall, for a Research on the Oxidation of various Substances (Salts, Acids, &c.) by means of Electrolysis	25
W. H. Pendlebury, for the Investigation of a Case of Gradual Chemical Change, namely, that between Potassium Chromate and Potassium Iodide in presence of an Acid	15
J. A. Harker, to investigate further the Change of Volume which occurs in the Combination of Chlorine and Hydrogen, and to find out its Cause; and for two other specified Researches	50
Carried forward........	£1,760

	£
Brought forward	1,760
W. A. Shenstone, for Payment of an Assistant in investigating (1) the Influence of the Silent Discharge of Electricity on Gases; (2) the Conditions of the Formation of Haloid Salts.	50
Dr. C. R. A. Wright, for Continuation of Experiments on "Ternary Alloys," more especially those containing Lead (or Bismuth) and Zinc as Immiscible Metals with Cadmium (or Antimony) as "Solvent" Metal.	50
Dr. A. W. Bishop, to continue the Investigation of the Compounds of Camphor-aldehyde ($C_{11}H_{16}O_2$) already begun in conjunction with Prof. Claisen, of Munich	20
Dr. H. G. Colman, for a Research on the Action of Acetobutyl Bromide on Ethyl Malonate	20
Dr. T. R. Marshall, for completing a Research on the Constitution of Trimethylene Derivatives, and for other specified Researches	25
Prof. W. R. Dunstan, for a Research on the Action of Alkalies on the Nitro-paraffins	50
S. U. Pickering, for Continuation of his Research on the Nature of Solutions	25
Dr. F. S. Kipping, for a Study of Fluorescent Compounds..	25
H. N. Dickson, for an Investigation of the Physical Condition of the Waters of the English Channel	100
J. Murray, for further Examination of the Western Lochs of Scotland	300
Western Scotland Marine Flora Committee (per G. Murray), for the Exploration of the Marine Flora of Western Scotland.	100
Prof. T. Johnson, for an Investigation of the Marine Flora of Ireland, especially the West and South-west Coasts	30
J. M. Macfarlane, for a further Study of Plant Hybrids....	25
W. T. Thiselton Dyer, for a Collector to be attached to the Sierra Leone Delimitation Commission	350
T. W. Bridge, for further Investigations into the Anatomy of the Teleostean Fishes	20
A. Willey, for an Investigation of the Anatomy of *Balanoglossus* (*Extended Grant*, £150 *for two years*)	300
Liverpool Marine Biology Committee (per W. A. Herdman), towards the Expenses of the further Exploration of the Marine Fauna and Flora of Liverpool Bay	50
Prof. A. C. Haddon, for an Investigation on the Anatomy of the Actiniæ and Corals of Torres Straits	50
Carried forward........	£3,350

	£	s.	d.
Brought forward	3,350	0	0
Dr. D. Sharp (for a Committee), to report on the present State of our Knowledge of the Sandwich Islands, and to investigate ascertained Deficiencies in the Fauna	200	0	0
W. Garstang, to obtain material for working out the Embryology of certain Tunicata	50	0	0
T. Scott, for Study and Description of the Entomostraca obtained by Mr. Rattray in 1886............	20	0	0
Secretary, Royal Society, for Balance of Account for Objectives purchased in pursuance of a Resolution of Board G, and engraving the same	5	11	6
Dr. L. Shore, for an Investigation of the Action of the Epithelial Cells of the Intestinal Mucous Membrane on Peptone during Absorption	20	0	0
Dr. McFadyean and Dr. A. P. Aitken, for a Research on the Toxic Substances generated by the Microbes of Anthrax, Black-quarter, and Glanders, and on a "Chemical Vaccine" for each of these Diseases	75	0	0
C. S. Sherrington, for the Examination in detail of the Actions and the Topography of Reflex and Automatic Centres in the Lower Half of the Spinal Cord, &c.	75	0	0
Prof. Schäfer, for (specified) Investigations into the Functions of the Central Nervous System in Monkeys and Dogs	75	0	0
Research Committee, Pharmaceutical Society (per Prof. W. R. Dunstan), for an Investigation of the Nature of the Alkaloids contained in the various Species of Aconite	150	0	0
G. N. Stewart, for Investigation of the Vasomotor Regulation of the Circulation in particular Organs and Parts of the Body	10	0	0
Dr. P. F. Frankland, for continuing his Investigations on the Chemical Changes brought about by specific Micro-organisms	80	0	0
L. Hill and W. M. Bayliss, for a Research on the Formation of Heat in Secreting Glands and the Influence of the Nervous System thereon	25	0	0
W. Saville Kent, for a Research on Corals and Coral Animals in the Fiji Islands	100	0	0
Prof. D. E. Jones, in aid of an Investigation on Hertzian Vibrations	35	0	0
J. H. Cooke (per J. Murray), in aid of a Geological Investigation in Malta and adjacent Islands	20	0	0
	£4,290	11	6

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance, November 30, 1890	460	18	8	By Appropriations, as above	4,290	11	6
,, Grant from Treasury	4,000	0	0	,, Salaries, Printing, Postage, Advertising, and other Administrative Expenses	93	15	1
,, Repayments	240	6	8	,, Balance, Nov. 30, 1891	373	14	3
,, Interest on Deposit	56	15	6				
	£4,758	0	10		£4,758	0	10

Account of Grants from the Donation Fund in 1890–91.

	£	s.	d.
Prof. Haswell, towards the Institution of a Marine Biological Station at Sydney	50	0	0
Dr. Sclater, to enable Capt. Swayne to procure him Specimens of African Antelopes	20	0	0
Dr. Woodward, to aid Dr. Forsyth-Major in his Researches on Fossil Mammalia	50	0	0
Dr. Woodward, in aid of the Publication of the Researches of Dr. Forsyth-Major on Miocene Fossil Mammals of the Island of Samos	100	0	0
Prof. Haddon, in aid of the Publication of his Ethnographical Researches in the Torres Straits	50	0	0
Mr. Carruthers, in aid of Mr. E. J. Baker's Visit to the Herbaria of Madrid and Geneva	15	0	0
A. Soper, Balance of the Grant of £200 to the late W. de la Rue for completing his Catalogue of Latitude and Longitude of Solar Spots	17	13	0
	£302	13	0

December 10, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents—

The Treasurer.
Professor G. C. Foster.
Professor Liveing.
Sir G. G. Stokes.

The President read the following Letter from Professor Dewar:—

Royal Institution,
10th December, 1891.

DEAR SIR WILLIAM THOMSON,

The following observation, which I have just made, may interest the members of the Royal Society, and if you think it of sufficient importance you may announce it at this day's meeting.

At 3 P.M. this afternoon I placed a quantity of liquid oxygen in the state of rapid ebullition in air (and therefore at a temperature of $-181°$ C.) between the poles of the historic Faraday magnet, in a cup-shaped piece of rock salt (which I have found is not moistened by liquid oxygen, and therefore keeps it in the spheroidal state), and to my surprise I have witnessed the liquid oxygen, as soon as the magnet was stimulated, *suddenly leap up to the poles and remain there permanently attached until it evaporated.* To see liquid oxygen suddenly attracted by the magnet is a very beautiful confirmation of our knowledge of the properties of gaseous oxygen.

Yours faithfully,
JAMES DEWAR.

The following Papers were read:—

I. "On a Compensated Air Thermometer." By H. L. CALLENDAR, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received October 29, 1891.

In a paper which I had the honour to present to the Royal Society some four years ago "On the Practical Measurement of Tempera-

ture,"* I described in detail a somewhat elaborate form of air thermometer with which it was found possible to attain an accuracy of the order of 0·01° C. I have since succeeded in overcoming some of the difficulties encountered in that investigation, and in evolving on similar lines a form of instrument which is capable of a much higher order of accuracy, and which has the further advantage that both the observations and the calculations are immensely simplified.

The standard instrument for measuring temperature selected by Regnault in his classical researches was the constant-volume air thermometer. In my earlier experiments I employed air thermometers of this type, but modified them by the introduction of a sulphuric acid gauge of small bore between the thermometric bulb and the mercury manometer. I was thus enabled to reduce the correction to be applied for the small volume of air which was not exposed to the temperature to be measured, and at the same time to observe small variations of temperature with greater accuracy.

The constant-volume type of air thermometer, however, has several disadvantages. The degree of accuracy attainable depends primarily on the exact measurement of pressure by means of a mercury manometer. The observations involved are slow and laborious, and it is difficult, unless the temperature is absolutely steady, to secure an accuracy of the order of a tenth of a degree C. At high temperatures this method has the further disadvantage that the bulb is exposed to variations of pressure, the effect of which in altering its volume cannot be accurately estimated.

For these and other reasons I soon abandoned the constant-volume air thermometer in favour of the constant-pressure type. The bulb may thus be entirely freed from stress at high temperatures, and the mercury manometer may be dispensed with. The auxiliary reservoir containing mercury into which the air is allowed to dilate may be kept permanently in melting ice, and the volume representing the dilatation of the air may be determined by weighing the mercury displaced. This observation may be made at leisure, and admits of very considerable accuracy.

With the form of instrument described in the previous paper, it was still necessary to read the barometer. These readings were found to be by far the greatest source of uncertainty. I was so much impressed with this in the course of some experiments on the boiling-point of sulphur† that I determined to construct an instrument which should be altogether independent of the measurement of mercury columns.

If the pressure of the air enclosed, instead of being adjusted to equality with that of the atmosphere, be adjusted always to the same

* 'Phil. Trans.,' A, 1887.

† 'Phil. Trans.,' A, 1891, p. 130.

standard constant pressure, the trouble of reading the barometer will be saved, and the calculations will be considerably simplified. The simplest, and at the same time the most accurate, method of securing a standard constant pressure is to connect the outer limb of the sulphuric acid gauge to a glass bulb filled with air of suitable density, and kept in melting ice, preferably in the same receptacle as the mercury bulb in which the dilatation of the air is measured. The pressure in the thermometric bulb can easily be adjusted to equality with the standard pressure to within 1 or 2 mm. of sulphuric acid, and the small outstanding difference of pressure can be read quickly and accurately by means of a kathetometer microscope.

I set up an experimental instrument of this kind in October, 1890, and satisfied myself that it was quite possible to read its indications to the thousandth part of a degree C. at ordinary temperatures. This, I believe, to be a very much higher order of accuracy than has hitherto been thought attainable with an air thermometer. My only remaining difficulty was the slight uncertainty as to the mean temperature of the connecting tubes. By the use of the sulphuric acid gauge it is possible to make this correction comparatively small, but it still remains uncertain, and varies slightly with the extent of immersion of the stem of the thermometer. It is also a rather troublesome correction to apply, and complicates all the calculations very considerably.

It has since occurred to me that this troublesome and uncertain correction may be entirely eliminated, both from the observations and from the calculations, with this particular form of instrument, by making the standard pressure bulb communicate with a set of connecting tubes equal in volume and similarly situated to those of the thermometric bulb itself.

The method of determining the correction to be applied for that part of the stem of which the temperature is variable, by means of a similar compensating tube placed in close proximity to it, has occasionally been applied by previous observers. It was first employed by Deville and Troost in 1864, in their experiments on the expansion of porcelain at high temperatures. They connected the compensating tube to a separate manometer, and by observing the pressure or the amount of the air it contained were enabled to eliminate the term representing the effect of the connecting tubes from their equations.

Other observers have used a similar device, but, so far as I am aware, no one has hitherto noticed that, in the case of the differential air thermometer, the compensation can be rendered automatic, so that changes of temperature of the connecting tubes have no effect on the readings, and need not be taken into account in the calculations.

The conditions under which the compensation is perfect with the

form of instrument above described are very simple. They are, (1) that the two sets of connecting tubes should be of equal volume and at the same mean temperature; (2) that the mass of air enclosed in the standard pressure bulb should be equal to that in the thermometric and mercury bulbs; (3) that the pressures should be adjusted to equality.

Let m_0 be the mass of air in the standard pressure bulb and its connecting tubes, and let p_0 be its pressure. Let V_0 be the volume of the bulb, and θ_0 its temperature measured on the scale of the air thermometer. Let v be the volume of the connecting tubes, and θ' their mean temperature; then we have

$$p_0 \{V_0/\theta_0 + v/\theta'\} = m_0 k,$$

where k is a constant.

Let m_1 be the mass of air in the thermometric and mercury bulbs and their connecting tubes, and let p_1 be its pressure. Let V_1, V_m be the volumes of the air in these bulbs respectively at the temperatures θ_1 and θ_m; then we have as before

$$p_1 \{V_1/\theta_1 + V_m/\theta_m + v/\theta'\} = m_1 k.$$

If now we make $m_0 = m_1$, and $p_0 = p_1$, we have the equation

$$V_0/\theta_0 + v/\theta' = V_1/\theta_1 + V_m/\theta_m + v/\theta'.$$

The term v/θ' disappears from the equation, and if we also make $\theta_0 = \theta_m$ by keeping both the mercury and standard pressure bulbs in melting ice, the value of θ_1 is *accurately* given by the very simple expression

$$\theta_1 = V_1\theta_0/(V_0 - V_m).$$

It is convenient, and at the same time more symmetrical, to make the volume of the standard pressure bulb adjustable with mercury. It is then possible to take observations with the same thermometer at different pressures. By this means, as explained in a previous paper,* we can investigate with some accuracy the behaviour of gases at high temperatures, and thus reduce the indications of the air thermometer to the true scale of absolute temperature.

The form of instrument above described is designed for the most accurate work. For rough purposes, and especially for small ranges of temperature, very much simpler instruments may be constructed on similar principles.

It is evident, from an inspection of the equations already given, that the compensation is still sufficiently accurate for rough work, provided that the difference of pressure is small, and that the volume

* 'Phil. Trans.,' A, 1887, p. 223.

of the connecting tubes is not too large compared with that of the bulb. It is often a matter of great convenience to have the thermometric bulb at some distance from the indicating apparatus, and not rigidly connected to it. Since the connecting tubes are compensated, they may be made of considerable length and of flexible material, such as compo. or even rubber tubing, without much loss of accuracy.

For moderate ranges of temperature, an auxiliary bulb for measuring the dilatation of the air and adjusting the pressures to equality may be dispensed with. The sulphuric acid gauge itself may be graduated to indicate the difference of temperature between the two bulbs. In ordinary work, however, it would be inconvenient either to keep the standard bulb always at the same temperature, or to take its temperature and do an addition sum at each observation. The simplest way of avoiding this is to adjust the volume of the sulphuric acid in the pressure gauge so that its expansion may compensate for the dilatation of the air in the standard pressure bulb. This compensation may readily be made sufficiently accurate over the small range of temperature of the air of a workshop or laboratory.

When the instrument is thus compensated for changes of temperature in the standard bulb, one tube of the pressure gauge can be graduated directly in degrees to indicate the temperature of the thermometric bulb. The indications are then as easy to read as those of a mercury thermometer. They are not affected by changes of temperature in the surrounding air or by variations in the height of the barometer, and they are independent of the length of stem immersed. The range covered by a single instrument may be 100° C. or more, and may be made to correspond to any part of the scale by suitably adjusting the volumes of the air bulbs and tubes of the pressure gauge.

I have found such thermometers* exceedingly convenient and satisfactory for rough work at temperatures beyond the range of mercury thermometers. They can be made to read easily to the tenth of a degree at 450° C., and if properly compensated their indications are very reliable. Such a degree of accuracy is amply sufficient for most purposes, and the absence of all necessity for calculation or correction of the readings is a very great advantage.

* Perhaps I ought to mention that this direct-reading form of instrument has been patented, owing to its many commercial applications. It is made by Mr. J. J. Hicks, of Hatton Garden, E.C.

II. "Note on the Necessity of using Well-Annealed and Homogeneous Glass for the Mirrors of Telescopes." By A. A. Common, LL.D., F.R.S. Received November 18, 1891.

In 1880 I ordered of the St. Gobain Glassworks, through their London agent, M. de Grand Ry, a disk of glass for the mirror of a 5-foot telescope. The limit of weight imposed by the manufacturers permitted a disk of about 61 inches diameter and 5 inches thickness; this was made with a hole through the middle of 10 inches, in order to enable the telescope to be used as a Cassegrain telescope if required.

Not being in a position to begin work at that time, the disk of glass was left in its case, standing against a wall, at a slight angle, till 1886, when it was put upon a grinding machine to be worked into a mirror. With the intention of acquiring the necessary skill to make a good mirror, I intended to make many mirrors of this one disk by successive re-grinding and re-figuring.

The first polishing was done before the whole surface of the glass was brought down to a uniform face.

On inspection by Foucault's method of testing at the centre of curvature, the image of a round hole was found to be very elliptical; very little attention was paid to this at the time, as it was thought that subsequent work would bring all right. After many re-grindings, in each of which a practically new mirror was made, this elliptic appearance of the image persisted. Local polishing was tried, to improve the figure of revolution, without success, in fact it made matters worse. The telescope mounting being ready in 1888, the mirror was finished as well as possible, and put into the telescope, where a star could be examined, this not being possible when the mirror was on the machine; for photographic purposes the mirror was fair, giving good stellar images, but for visual work, with moderate powers, the definition did not come up to the required standard.

It was of interest to find the cause of this bad image; at first it was thought that the long time the disk had stood on edge, at an angle, had caused it; in this case it might in time become less; but, after being re-made in 1889, and again in 1890, the mirror was, if anything, worse than before. When polished, in certain lights broad bands of colour, red and blue, could be seen in the body of the glass about a foot from the edge all round, indicating in my opinion much internal tension, probably due to imperfect annealing.

Another disk was ordered in December, 1888, as soon as the first had been tried in the telescope. This second disk was delivered in 1890 and made into a mirror, without showing in the slightest manner any of the defects of the first.

In the course of the work on this last disk, a discovery was ma

that most probably explains the cause of failure of the first; in the process of polishing a certain amount of heat is produced, and it was always the custom to allow the mirror to cool down for some hours before testing; it was also always considered that some slight change of focal length was caused by the heat, but I was quite unprepared, on testing the mirror directly after polishing, in order to determine what this change amounted to, to find the enormous amount thus produced. After two hours figuring with a 15-inch polisher, with the face of harder resin than usual, so that the friction and heat were below the average amount in one polishing, the change of focal length was found to be 4 inches, that is to say, the image was made 4 inches further from the mirror than the usual place, or the mirror had at that time a radius of curvature 2 inches greater than the normal; in the course of three hours this had disappeared, and the image was produced at the normal place. As the whole trouble of the first mirror was caused by opposite diameters of the same zone coming to a focus in planes differing by about $\frac{1}{20}$ of an inch, it can easily be seen that this may have been caused by the failure of the glass to contract in a perfect and regular manner; in figuring, a large amount of the work would necessarily be done on the expanded glass, and it is reasonable to suppose that the figure thus given would be correct while the glass was in this state, but that on cooling, unless the glass contracted regularly, this correct figure would be lost.

After the second mirror was finished the first mirror was re-ground, and the polishing was done very slowly, half-an-hour in the morning, and the same time in the evening, with manifest improvement in the figure, but still without getting rid of all the defects.

That the fault was in the glass there is no doubt; the method of working adopted shows, in the case of the second 5-foot, and in other mirrors of 20-, 30-, and 36-inch mirrors, that a perfect surface of revolution can always be obtained if the glass is good; unless this perfect surface of revolution is obtained, it is quite hopeless to expect a good mirror.

The makers could not re-anneal the first disk, but they have undertaken, in a very handsome manner, to replace it by a new one; this I hope will be as good as the second one made by them, which is as nearly as possible perfect.

III. "On some of the Properties of Water and of Steam." By WILLIAM RAMSAY, F.R.S., Professor of Chemistry in University College, London, and SYDNEY YOUNG, Professor of Chemistry in University College, Bristol. Received November 5, 1891.

(Abstract.)

This investigation forms one of a series, former members of which refer to the thermal properties of ethyl oxide and various alcohols. Owing to the high temperature of the critical point of water, the work was confined to comparatively low temperatures. Tables are given in the paper of the orthobaric volumes of liquid water at temperatures between 100° and 270°; of the compressibility of water at different temperatures; of the vapour-pressures of water up to 270°; and of the density of the vapour of water under various conditions of temperature and pressure. Regnault's measurements of vapour-pressure do not extend beyond 220°; and the results of this investigation confirm them in a remarkable manner, besides amplifying them. The densities of the saturated vapour, also, measured directly, are nearly identical with those calculated from Regnault's determinations of heats of vaporisation; but near the condensing point of steam, especially at low temperatures, the pressure is too low, owing to the adhesion of water-vapour to glass, which causes condensation at pressures below the vapour-pressures. This necessarily renders the measurements near the condensing points uncertain, but the numbers calculated from Regnault's results give volumes for saturated steam agreeing sufficiently well with those obtained by direct measurements at volumes somewhat larger than those of the saturated vapour. It is probable that the real isochoric lines for water show a linear relation between temperature and pressure; but, owing to the circumstance mentioned, they deviate from rectilinearity near the condensing-points of the vapour.

IV. "On Hindoo Astronomy." By W. BRENNAND. Communicated by C. B. CLARKE, F.R.S. Received November 10, 1891.

(Abstract.)

Introduction.—Gives a short history of Indian astronomy, as known in Europe in the last century.

Treats of Indian astronomical æras, and gives some account of the Siddhantas and their authors, &c.

Ends with an investigation of the great number called the Kalpa, of 4,320,000,000 years, showing its uses in astronomical calculations,

and that there is concealed within it, as a sacred mystery, the true value of the precession of the Equinox.

Chapter I.—On the revolutions of the celestial bodies; their mean sidereal and synodic periods, as compared with the same elements in modern tables. Mean places at a given time. On the Earth's diameter, &c. On the Moon's horizontal parallax and distance from the Earth.

A theory deducing the orbits of the planets and the extent of the Universe, or Brahmanda, from the Moon's daily rate of motion in her orbit.

Theory regarding the causes of the planetary motions, &c.

Chapter II.—On trigonometrical formulæ known to the Hindoos. The construction of their tables of sines and versed sines. On the epicycle and its deferent, and on the eccentric and concentric, used for calculating the "true" place of a planet from the mean place.

Chapter III.—Problems in astronomy, on time, ascensional difference, declination, celestial longitude, horoscope, &c.

Chapters IV, V, VI.—The calculation and projection of lunar and solar eclipses.

Conjunctions, heliacal risings and settings, stars of the Zodiac, &c., The lunisolar year, &c.

The cycle of Jupiter of 60 years.

V. "Repulsion and Rotation produced by Alternating Electric Currents." By G. T. WALKER, B.A., B.Sc., Fellow of Trinity College, Cambridge. Communicated by Prof. J. J. THOMSON, F.R.S. Received November 5, 1891.

(Abstract.)

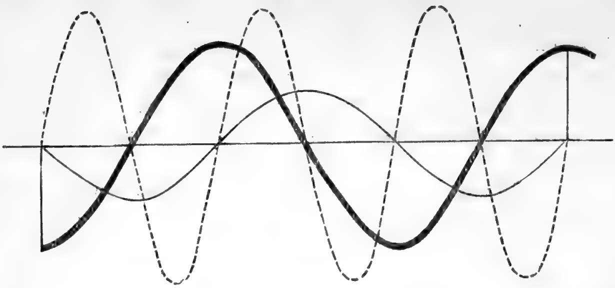
In the 'Electrical World,' May, 1887, p. 258, or the 'Electrical Engineer' (New York), June, 1887, p. 211, "Novel Phenomena of Alternating Currents," may be seen an account of some experiments by Professor Elihu Thomson on the mechanical force between conductors in which alternating currents are circulating.

In the case of a ring of metal in the presence of an electromagnet, in the coils of which an alternating current is passing, a force of repulsion is experienced by the ring, which may be explained as follows:—

Were the induced currents in the closed conductor unaffected by self-induction, the only phenomena exhibited would be alternate equal attractions and repulsions.

This may be illustrated by fig. 1. Here the strong line represents the primary and the thin the secondary, while of the dotted line any

Fig. 1.

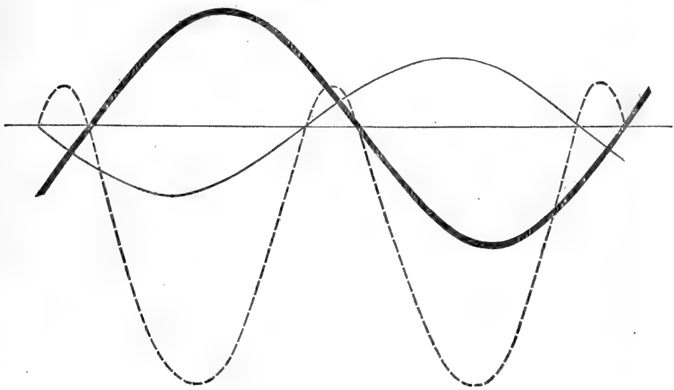


ordinate represents the product of the ordinates of the other lines, and hence represents the mechanical force of attraction or repulsion.

In the case of self-induction causing a lag, shift, or retardation of phase of the secondary current, there is a repulsion due to the summative effects of strong opposite currents for a lengthened period against an attraction due to the summative effects of weak currents of the same direction during a shortened period, the resultant effect being a greatly preponderating repulsion.

The diagram for this is fig. 2. Professor Thomson has shown, experimentally, that two circular coils, whose planes are perpendicular

Fig. 2.



to the line joining their centres, repel one another when an alternating current traverses one of them, and that if they be placed with their centres coincident and planes making an acute angle, there will be a couple tending to increase that angle.

As these results have been used as means of measuring alternating

currents, I have calculated in § 1 expressions for the intensities of the force and couple respectively.

Another experiment is the following:—A sheet of copper is placed so as to half cover an alternating magnetic pole. Upon this, near the pole, is laid a hollow sphere of copper. The electromagnetic action produces a couple so powerful that the friction of rotation is overcome, and the sphere spun round.

In order to throw light on this, after a theorem in § 2 as to the kind of currents set up in a conductor, I have considered a number of cases. A thin circular infinite cylindrical shell lies in an alternating field of currents parallel to its axis, and the couple upon it is found. The result is applied to give the couples on two such shells in the presence of a parallel current and of a pair of currents forming an electromagnet.

The couple in action upon a thin spherical shell in a general periodic field, has next been found, and is applied to give the couples on two thin shells under the influence of—

(i.) An alternating current in a straight infinite wire.
(ii.) A pair of such currents forming an electromagnet.
(iii.) An alternating magnetic pole.
(iv.) An alternating electromagnet of very short length.

It transpires that, whatever the field in action upon the cylinder and sphere (the former consisting of currents parallel to the axis), there will be no couple if the field be completely in one phase. Thus, the sphere of Professor Thomson is made to spin because the currents induced in the copper plate do not coincide in phase with those of the magnet, and not (as has been stated) because it acts as a screen and renders the field unsymmetrical. Were the plate a perfect conductor, it would be a perfect screen, but there would be no couple.

Presents, December 10, 1891.

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December 17, 1891.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Chairman read the following letter from Professor Dewar :—

Royal Institution,
17*th December*, 1891.

DEAR SIR WILLIAM THOMSON,

I had intended coming to the Society, in order to make a further communication with regard to the magnetic and other properties of liquid oxygen, but I am confined to the Laboratory, owing to difficulties with regard to the progress of such investigation. In the meantime it may interest the Fellows to know that I have examined the properties of liquid *ozone* in the magnetic field, and find it also highly attracted.

I hope to make a detailed communication very soon to the Society.

Yours faithfully,
JAMES DEWAR.

The following Papers were read :—

I. "The 'Ginger-beer Plant,' and the Organisms composing it: a Contribution to the Study of Fermentation-yeasts and Bacteria." By H. MARSHALL WARD, M.A., F.R.S., F.L.S., Professor of Botany at the Forestry School, Royal Indian Engineering College, Cooper's Hill. Received November 19, 1891.

The author has been engaged for some time in the investigation of a remarkable compound organism found in home-made ginger-beer fermentations.

It occurs as jelly-like, semi-transparent, yellowish-white masses, aggregated into brain-like clumps, or forming deposits at the bottom of the fermentations, and presents resemblances to the so-called *Kephir* grains of the Caucasus, with which, however, it is by no means identical.

He finds that it consists essentially of a symbiotic association of a specific *Saccharomycete* and a *Schizomycete*, morphologically comparable to a Lichen, but, as met with naturally, invariably has other species of yeasts, bacteria, and mould-fungi casually associated with these.

He has successfully undertaken the separation of the various forms, and groups them as follows:—

1. The essential organisms are a yeast, which turns out to be a new species allied to *Saccharomyces ellipsoideus* (Reess and Hansen), and which he proposes to call *S. pyriformis;* and a bacterium, also new and of a new type, and named by him *Bacterium vermiforme.*

2. Two other forms were met with in all the specimens (from various parts of the country and from America) examined—*Mycoderma cerevisiæ* (Desm.) and *Bacterium aceti* (Kützing and Zopf).

3. As foreign intruders, more or less commonly occurring in the various specimens examined, were the following:—

α. A pink or rosy yeast-like form—*Cryptococcus glutinis* (Fresenius.)?

β. A small white aërobian top-yeast, with peculiar characters, and not identified with any known form.

γ. The ordinary beer-yeast—*Saccharomyces cerevisiæ* (Meyen and Hansen).

δ. Three, or probably four, unknown yeasts of rare occurrence.

ε. A bacillus which forms spores, and liquefies gelatine with a greenish tinge.

ζ. A large spore-forming bacillus, which also liquefies gelatine.

η and *θ*. Two—perhaps three—other Schizomycetes not identified.

ι. A large yeast-like form which grows into a mycelium, and turns out to be *Oïdium lactis* (Fresenius).

κ. A common blue mould—*Penicillium glaucum* (Link).

λ. A brown "Torula"-like form, which turns out to be *Dematium pullulans* (De Bary).

μ. One, or perhaps several, species of "Torula" of unknown origin and fates.

Of these forms, the author has succeeded in cultivating and examining very thoroughly all but those under *θ* and *μ* in the foregoing list.

Saccharomyces pyriformis (n.sp.) is a remarkably anaërobian bottom-yeast, forming spores, and developing large quantities of carbon dioxide, but forming little alcohol. It has also an aërobian form—veil form of Hansen—in which the rounded cells grow out into club-shaped or pyriform cells, whence the proposed specific name. It inverts

cane sugar, and ferments the products; but it is unable to ferment milk sugar. It forms rounded, morula-like, white colonies in gelatine, and the author has separated pure cultures from these. He has also studied the development and germination of the spores, which are formed in 24—48 hours at suitable temperatures on porous earthenware blocks. They also develop on gelatine.

The technological characters have been kindly determined and confirmed for the author by Mr. Horace Brown, F.R.S., and Dr. Morris, of Burton-on-Trent.

The specific Schizomycete (*Bacterium vermiforme*, n.sp.) has been very fully studied by the author. It occurs in the fermentations as rodlets or filaments, curved or straight, encased in a remarkably thick, firm, gelatinous sheath, and is pronouncedly anaërobic, so much so, that the best results are got by cultivating it in carbon dioxide under pressure.

The sheathed filaments are so like worms, that the name proposed for the species is appropriately derived from this character.

It will not grow on gelatine, and separation cultures had to be made in saccharine liquids by the dilution methods.

It grows best in solutions of beet-root, or of cane sugar, with relatively large quantities of nitrogenous organic matter—*e.g.*, bouillon, asparagin—and tartaric acid. Good results were obtained with mixtures of Pasteur's solution and bouillon.

The author found that the bacterium into which the filaments subsequently break up can escape from its sheath and become free, in which state it divides rapidly like ordinary bacteria. Eventually, all the forms—filaments, long rods, short rodlets—break up into cocci. No spores have been observed. These changes are dependent especially on the nutritive medium, but are also affected by the gaseous environment and the temperature. The jelly-like clumps of the so-called "ginger-beer plant" are essentially composed of these sheathed and coiled Schizomycetes, entangling the cells of *Saccharomyces pyriformis*. But the action of the Schizomycete alone on the saccharine medium differs from that exerted when it is associated with the yeast, and from that exerted by the latter alone.

This was proved by cultivating each separately, and also by cultivations in which, while each organism was submerged in the same fermentable medium, they were separated by permeable porcelain (Chamberland filters), through which neither could pass.

The author has also reconstructed the "ginger-beer plant" by mixing pure cultures of the above two organisms; the Schizomycete entangled the yeast-cells in its gelatinous coils, and the synthesised compound organism behaved as the specimens not analysed into their constituents.

Some very curious phenomena in connexion with the formation of

the gelatinous sheaths and the escape of the bacteria from them were observed in hanging-drop cultures, and are figured and described by the author. The conditions for the development of the gelatinous sheaths—and therefore of the coherent brain-like masses of the Schizomycete—are a saccharine acid medium and absence of oxygen. The process occurs best in carbon dioxide : it is suppressed in bouillon, and in neutral solutions in hydrogen, though the organism grows in the free, non-sheathed, motile form under these conditions.

The behaviour of pure cultures of the bacteria in as complete a vacuum as could be produced by a good mercury pump, worked daily, and even several times a day for several weeks, is also noteworthy. The author records his thanks to his friend and colleague Professor McLeod for much assistance in regard to this apparatus. The development of the sheaths is apparently indefinitely postponed *in vacuo*, but the organism increased, and each time the pump was set going an appreciable quantity of carbon dioxide was obtained. In vacuum tubes the same gas was evolved, and eventually attained a pressure sufficient to burst some of the tubes. The quantity of carbon dioxide evolved daily by the action of the bacterium alone, however, is small compared with that disengaged when the organism is working in concert with the symbiotic yeast ; in the latter case the pressure of the gas became so dangerous that the author had to abandon the use of sealed tubes.

The products of the fermentation due to the Schizomycete have not yet been fully determined in detail ; lactic acid, or some allied compound, seems to be the chief result, but there are probably other bodies as well.

The author owes acknowledgment to Dr. Matthews, of Cooper's Hill, for advice and assistance in examining the products of these fermentations.

The pink yeast-like form proved to be very interesting. It has nothing to do with the "ginger-beer plant" proper, though it was invariably met with as a foreign intruder in the specimens. The author identifies it with a form described by Hansen, in 1879;* unfortunately the original is in Danish, but the figures are so good that little doubt is entertained as to the identity. It is also probably the same as Fresenius' *Cryptococcus glutinis* in one of its forms. It is not a Saccharomycete, and does not ferment like a yeast; it is aërobian.

The chief discovery of interest was that in hanging drops the author traced the evolution of this "rose-yeast" into a large complex mycelium, bearing conidia, and so like some of the Basidiomycetes that it may almost certainly be regarded as a degraded or

* 'Organismer i Öl og Ölurt,' Copenhagen, 1879.

"torula" stage of one of these higher fungi. Full descriptions and figures are given by the author.

The form *Mycoderma cerevisiæ* was thoroughly examined. The author's results confirm what is known as to its aërobian characters. Statements as to its identity with *Oïdium lactis* were not only not confirmed, but the author grew these two forms side by side, and maintains their distinctness. Nor could he obtain spores in this fungus, thus failing to confirm earlier statements to the contrary. He regards it as probable that oil-drops have been mistaken for spores; he also finds that in later stages of fermentation by this organism a strong oily-smelling body is produced.

With regard to *Bacterium aceti*, the author has nothing new to add. A point of some interest was the repeated production of acetic ether, which scented the laboratory when this Schizomycete was growing in company with the small white aërobian top-yeast referred to under (β). As this phenomenon was found to have nothing to do with the question being investigated, the author did not pursue it further. It seemed probable, however, that the yeast produced alcohol, which the Schizomycete, in presence of oxygen, partially oxidised, and that the fragrant ether was produced by interaction of the products.

With regard to the other forms found, the author was chiefly concerned with testing their relations to the important and essential organisms. It need only be remarked here that hanging-drop cultures of *Dematium pullulans* were very successful, and that some of the moulds, and at least one *bacillus* (of which the spore-formation, &c., were traced also), were traced to the ginger used in the manufacture of the well-known beverage.

The author hopes very shortly to have the honour to lay before the Society a full account of his research, of which the above is only a brief notice. The fuller account will contain detailed descriptions, as well as figures of the apparatus, mode of culture, &c.

II. "Studies in the Morphology of Spore-producing Members. Preliminary Statement on the Lycopodinæ and Ophioglossaceæ." By F. O. Bower, F.R.S. Received November 27, 1891.

It is currently held that the sporophyte, or neutral generation in archegoniate plants, is the result of elaboration of the zygote: that while in certain Algæ the zygote simply divides to form a number of spores (carpospores), in the lower Bryophyta there has been a differentiation of an external, sterile, and protective wall, distinct from the

internal mass of spores. The higher Bryophyta show a larger proportion of sterile tissue, which in them composes the seta, columella, and wall, while the sporogenous tissue is comparatively reduced in bulk, though still forming one united band. In the vascular plants the proportion of sterile tissue to the sporogenous tissue is larger still; elaboration of form has in them resulted in the production of appendicular organs, while the sporogenous tissue is partitioned off into small, isolated masses, or even single cells, these being situated in members which are commonly called *sporangia*. The series from the lowest to the highest of these types probably illustrates the essential points in the actual process of evolution of the higher from the lower forms; from it we recognise that the ascending series shows a *progressive sterilisation* of the tissues of the neutral generation, and also an increasing *elaboration of external form and internal structure*, the two lines of progress going, in a measure, hand in hand.

It is obvious that, if the progression were as above stated, the function of the spore-production preceded the vegetative functions of the sporophyte in point of time; spore-producing members may, in this sense, be termed *primary* from the point of view of descent, and the vegetative members, *secondary;* the morphology of spore-producing members should accordingly take precedence of the morphology of vegetative members, and an exhaustive study of the former is therefore specially necessary.

The widest gap in the series of those plants which show antithetic alternation is believed to be that between the Bryophyta and the Vascular Cryptogams; to bridge over this gap between plants with simple form and united archesporium, and those with complex form and separate small archesporia, is the most clearly outstanding problem of morphology. Of intermediate forms there are practically none known; but it is believed by the author that a careful examination of the spore-producing members of the lower vascular plants, with special regard to their development, will best lead to some clear opinion as to the way in which the transition may have taken place. A comparison of such plants as form natural series may demonstrate progressions of elaboration and sterilisation, which may be regarded as *analogous* to the progression from the Bryophyta to the Vascular Cryptogams. Such an analogous progression is believed to be found in the Lycopodinæ and Ophioglossaceæ. Even if this analogy be not admitted, the further investigation of the sporangia of certain rarer and less known Vascular Cryptogams, to be described below, will be of sufficient interest to justify the work.

The present preliminary statement will refer to the Lycopodinæ and Ophioglossaceæ only.

The simplest known type of the former is *Phylloglossum*, which I agree with Dr. Treub in regarding as a truly rudimentary form.

The sporophyte consists of two parts:—(i) the *protocorm*, with its protophylls and roots, and (ii) the *strobilus*, with sporophylls and sporangia. The transition from (i) to (ii) is usually sudden, and without intermediate steps.

The sporangium of *Phylloglossum*, as regards its external form, is intermediate in character between that of *Lycopodium Selago* and *L. alpinum;* the archesporium consists of a single row of about six cells, only one of which appears in each radial section; in this, *Phylloglossum* is like *L. selago*. The whole strobilus also, when mature, is closely similar to that of a simple *Lycopodium*, and the development of the sporangium corresponds also in all essential points.

Several species of *Lycopodium* have been examined as regards the structure and development of the sporangium; previous investigators have figured only radial sections; it is obvious that radial, transverse, and tangential sections in various stages will be necessary for the complete description of the development of so large and complex a body; the result of comparison of such sections has been—(i) to acquire a clear knowledge of the form and composition of the archesporium, and (ii) to show that this varies in different species of the genus.

Two cases will be briefly described, viz., *L. Selago* and *L. clavatum*.

In the former the sporangium appears as a rather narrow, sharply convex outgrowth from the upper surface of the sporophyll; the archesporium consists of a single row of cells, of which one only appears in the radial section; the smallest number seen in the tangential sections is seven, which may possibly be referable in origin to three parent cells. While the archesporium increases largely with age, neither the stalk of the sporangium nor the sub-archesporial tissue increases greatly in bulk, so that the mature sporangium assumes the form of a slightly curved sausage, attached by a comparatively slender and long stalk.

In *L. clavatum* (to which also *L. alpinum* closely corresponds), the sporangium appears as a broad, only slightly convex out-growth; the archesporium does not consist of a single row of cells, but usually of *three rows:* thus three cells would appear in each radial section: both radial and transverse sections show that these are not readily referable in origin to a single parent cell. In tangential section it appears that ten or more cells may be present in each row. As the archesporium grows, the stalk remains short and bulky, while the sub-archesporial mass develops as a large pad of sterile tissue, which arches the sporogenous tissue convexly upwards. The whole sporangium when mature is thus strongly curved, and is inserted on the sporophyll by a short and massive stalk. These characters are important for comparison with *Ophioglossum*.

It may be added that, so far as observations have yet been made on

Selaginella, its sporangium appears to be comparable to the *Selago* type of *Lycopodium*, though with differences of detail, which need not now be specified.

The results obtained by observation of the Lycopodinæ have been used for purposes of comparison with the Ophioglossaceæ, and the result is a view as to the real nature of the so-called "fertile frond." This structure has long been a morphological crux, and various opinions have been held with regard to it, which severally make demands upon morphological faith. The theory now to be put forward is, *that the "fertile frond" is an elaborated and partitioned sporangium*, homologous with the smaller and non-partitioned sporangium of the Lycopodinæ. Developmental evidence will now be adduced in support of this theory.

The "fertile frond" of *Ophioglossum vulgatum* arises as an outgrowth of the upper surface of the "sterile leaf": not, it is true, at the base, but rather below the middle: the cell-divisions do not correspond in detail to those in the young sporangium of *Lycopodium*, but I do not think that, at the present day, this will be reckoned as a material ground for rejecting an homology. There is in fact, at first, a single initial cell, with rather irregular segmentation, but the apical growth of the elongated and upward directed "fertile frond" passes over shortly to the type with, apparently, four initials.

The origin of the archesporium and sporogenous tissue has been traced in *Ophioderma pendulum* by the help of material supplied through the kindness of Dr. M. Treub. In this plant, as in *Ophioglossum*, the sporangia are deeply sunk in the "fertile frond," and form a longitudinal series running along each lateral margin of it, and extending to the extreme apex. Transverse sections in the young state would thus show the archesporia at the most curved points of the elliptical section. In the youngest transverse sections which were observed, the archesporial tissue appears composed of many cells, and it is doubtful whether all be referable to a single parent cell; but, seeing that this section would correspond to what is seen in a radial or a transverse section of the sporangium of *Lycopodium*, and that there is a difference (*e.g.*, between *L. selago* and *L. clavatum*) as to the reference of the archesporium to a single cell in such sections in different species of that genus, this question cannot be considered as a vital one.

The archesporial tissue thus seen in transverse sections of the young "fertile frond," is found in tangential and radial sections to be *a continuous band*, which extends along each margin to the apex; it is believed to correspond to the curved band of archesporial tissue in *Lycopodium*, and may, therefore, be styled the *potential archesporium*. But whereas in *Lycopodium* the whole of this tissue forms spores, only parts of it develop as sporogenous tissue in *Ophioderma:* for, as

it grows older, the archesporial band becomes differentiated into (*a*) *sporogenous masses*, which soon are densely filled with protoplasm, and develop further into spores, as already known, and (*b*) *sterile tissues*, which intervene between these, and develop into the septa between the sporangia, together with part of the tapetum.

A similar continuous band of hypodermal tissue has been found in *Ophioglossum* also, and is believed to be the continuous *potential archesporium*.

If the tissue thus recognised in *Ophioderma*, and, with less certainty, in *Ophioglossum*, be the potential archesporium, and correspond to the curved band of archesporium in *Lycopodium*, then the whole "fertile frond" of *Ophioglossum* and *Ophioderma* must be homologous with the sporangium of *Lycopodium:* the central tissue of the "fertile frond" will be the counterpart of the sub-archesporial mass of *Lycopodium*, and the whole will illustrate *the result of elaboration, partial sterilisation, and consequent partitioning of the sporangium.*

But it will further be pointed out that the Ophioglossaceæ, which are a natural family with obviously close affinities, illustrate the progress of elaboration of the sporangium by still further steps within their own circle of affinity. This will appear on comparison of *Helminthostachys*, of which I have received well-preserved specimens, collected in Ceylon by Mr. J. Bretland Farmer. Here the "fertile frond" is not so simple as in *Ophioglossum;* its homology is, however, demonstrated by its position, and by the fact that its main features of early development are similar. The spore-producing parts are borne along the two lateral margins; but, instead of the large sporangia deeply sunk in the tissue, as in *Ophioglossum*, their place is taken in *Helminthostachys* by branched outgrowths, each of which may bear a number of smaller sporangia; these branched outgrowths may be styled provisionally the *sporangiophores.* They are not disposed with strict regularity, but are restricted to the margins of the "fertile frond," and thus topographically they replace the sporangia of *Ophioglossum:* functionally they do the same, for part at least of their tissue becomes sporogenous. The development of them has been traced, but it need not now be described in detail; it will suffice to make the general statement that it shows nothing incompatible with the theory above put forward. The sporangia correspond in their development and main features to those of *Botrychium.* A comparison of *Helminthostachys* with *Ophioglossum* would point to the conclusion that in the former is seen a further phase of *elaboration*, *partial sterilisation*, and *partitioning.* If the archesporium of *Ophioglossum* were further subdivided by sterilised partitions, and the several parts raised by vegetative outgrowth of tissue, so as to project beyond the general surface, the result would be something like that which is actually seen in *Helminthostachys.*

A somewhat similar view, though different in some details, may be held also in the case of *Botrychium.*

There are, however, other facts relating to both the Ophioglossaceæ and Lycopodinæ which are obvious externally, and which are, I think, most readily explained on an hypothesis of elaboration and partial sterilisation: these will now be briefly alluded to. In certain species of *Lycopodium* (*e.g.*, *L. Selago*) there is no marked difference between the foliage leaves and the sporophylls: in many other species the difference is only slight. It may be noted that in certain species (*e.g.*, *L. alpinum*) towards the base of the strobilus the young sporangia show gradual reduction in size, these leading on to cases of complete abortion. At the apex of the strobilus also there is often a similar reduction of the sporangia (*Phylloglossum*, *L. clavatum*, &c.), the uppermost leaves being thus sterile. Again in *L. Selago* and some tropical species, sporangia are borne on certain zones of the plant, while on alternating zones there are only sterile leaves. In the Ophioglossaceæ also the leaves of weak plants are frequently sterile, and bear no matured "fertile fronds": examination of these, however, frequently discloses a small and abortive "fertile frond" occupying the position of the normal one. How are these organs in the Lycopods and *Ophioglossum* to be explained unless they be regarded as the reduced and abortive remains of parts which under other circumstances might have come to functional maturity? They are, in fact, evidence of further *sterilisation*, though in this case it is not partial, but extends even to the whole sporogenous tissue of certain sporangia.

Putting together these last facts, and the developmental data previously stated, the following seems to me to be a reasonable theory, which will, I think, throw some light upon the probable relations of the Lycopodinæ and Ophioglossaceæ from the point of view of descent:—

The primeval strobilus consisted of axis, sporophylls, and sporangia: any of these parts was capable of further elaboration, and the balance of them *inter se* might thus have been modified.

In *Phylloglossum* there is a small strobilus which springs directly from the protocorm. This whole strobilus is probably of a primitive type, and may correspond to an elaborated sporogonial head (see below).

In the various species of *Lycopodium* the whole plant (exclusive of the protocorm, &c.) represents an extended and branched form of such a strobilus, of which many of the sporophylls have been sterilised, and appear as the foliage leaves, having no sporangia. The whole plant shows a prevalence of development of the axis over the leaves or sporangia.

In the Ophioglossaceæ the converse is the case; the axis remains relatively small (except in *Helminthostachys*, where it is seen as the

horizontal rhizome), while the sporophylls (= "sterile fronds") and sporangia (= "fertile fronds") are largely extended, the elaboration of the two showing a remarkable parallelism. In *Ophioglossum vulgatum*, where the sporophyll is simple though much larger than in *Lycopodium*, the sporangium ("fertile frond") is elongated and partitioned by the sterilisation of transverse bands of the potential archesporium. In *Ophioderma* (as also in some species of *Ophioglossum*) the sporophyll is irregularly lobed, while the sporangium ("fertile frond") is occasionally branched. In *Helminthostachys* the sporophyll is branched, and the whole sporangium ("fertile frond") is also branched occasionally; but in addition it shows that elaboration above described, and it may be looked upon as the result of further partitioning of the archesporium, and outgrowth of separate parts of the superficial tissues as the "sporangiophores." Finally, in some species of *Botrychium* the sporophyll and sporangium ("fertile frond") are both repeatedly branched, and show the furthest divergence of the whole series from their simpler prototypes, which are the sporophyll and sporangium of the Lycopods.

In this view thus stated there is nothing incompatible with what might be expected on *a priori* grounds; on the contrary, there is good reason to look upon such a progression as one of the natural ways in which the number of spores produced might be increased, such increase in number being obviously beneficial. Taking the simple strobilus as the starting point, one method would be the elongation of the strobilus, and increase in the number of sporangia produced upon it, they remaining individually of relatively small size; this is the type seen in the Lycopods. A converse method would be to increase the individual size of the sporangium, while the number of sporangia might remain small and be matured at intervals; this is exemplified by the Ophioglossaceæ. But if simple enlargement of the sporangium took place, without subdivision of the archesporium, the rapid supply of nourishment to the enlarged mass of growing spores would be difficult, while at the period when the sporogenous mass is semi-fluid, owing to the cells separating from one another, and floating freely in fluid, the sporangium would run great risk of mechanical injury from without, and a single puncture of the wall of the large sporangium would ruin the whole. These difficulties are all avoided by partitioning of the sporangia; the sterile tissue of the partitions, while strengthening the whole, would, together with the increased sub-archesporial mass, serve the more readily to bring nourishment to the developing sporogenous tissue; the transfer is further provided for by the vascular system which extends upwards through the centre of the "fertile frond," and even into the sporangiophores of *Helminthostachys*.

The effect of this theory would be to bring the Ophioglossaceæ

systematically nearer to the Lycopodinæ; this relationship has been recognised by various writers on other grounds, and the characters of the sexual generation will even help to support this nearer affinity.

A special interest will now centre round a small and very rare plant, viz., *Ophioglossum Bergianum*, one dried specimen of which I received from Professor MacOwan. It appears to show points of affinity both to *Phylloglossum* and *Ophioglossum*. While it is obviously an *Ophioglossum*, it shows (1) in the form of the leaf and the general habit, (2) in the fact that more than a single leaf is exposed at once, (3) in the low point of insertion of the "fertile frond," and (4) in the small number of partitions in it (sporangia, 8—12), characters which suggest that its further investigation would probably disclose facts of the greatest importance.

The relation of *Isoetes* to our series will naturally be a close one; just as the plant of *Ophioglossum* may be looked upon as a vertical strobilus, of which the large sporophylls are developed in slow succession, so also may *Isoetes* be regarded as a simple vertical strobilus, but the leaves are more numerous; the leaf (sporophyll) is relatively large, though simple. The sporangium is, in form, like a flattened cake, inserted on the upper surface of the leaf, near its base; it also is partitioned, more or less completely, and Goebel has shown clearly in his drawings how the hypodermal tissue, which may here also be styled the potential archesporium, becomes differentiated into sterile trabeculæ and sporogenous cells. To this differentiation I should assign a similar interpretation to that in the Ophioglossaceæ; the sporangium is, however, less elaborated, and its size and prominence are not such as to have led to its sporangial character being lost sight of.

Quite recently, on examining the fine series of sections of *Lepidostrobus* in the British Museum, I have found processes of sterile tissue which spring from the base of the sporangium, and project far into the mass of spores; these appear to be comparable to the trabeculæ of *Isoetes*, though differing in points of detail.

I do not think it desirable as yet to express opinions as to the bearing of this work upon the other Ferns. At present I am disposed to think that the Ophioglossaceæ and *Isoetes* are in an intermediate position between the Lycopods and other Ferns, and that their affinity to the former is certainly quite as close as to the latter.

The Psilotaceæ are probably a separate series, remote from both; they will be dealt with on a later occasion.

We have seen how the Ophioglossaceæ illustrate, according to our theory, the elaboration and partitioning of a sporangium, which in the Lycopods is relatively small and simple, and has an undivided archesporium. It is suggested that in this series there may be seen a progression *analogous* to that by which the vascular plants originated from some Bryophytic forms with simple sporogonial head. Take,

for example, the strobilus of *Lycopodium*, or of *Equisetum;* it is not difficult to see how, from a Bryophyte with an archesporium such as that of *Anthoceros*, the strobilus might originate from the sporogonial head, by partitioning off the archesporium (such as that seen in *Ophioderma*), and outgrowth of new members from the surface (such as are seen in *Helminthostachys*). The details of the process might doubtless be different; it is not even contended at present that this may have been the mode of origin of *all* the stocks of Vascular Cryptogams. But the point is that *an elaboration similar to that which may be traced in the spore-bearing members of the Lycopodinæ and Ophioglossaceæ of the present day might, if carried out in a sporogonium such as that of* Anthoceros, *result in a strobilus not unlike those of* Equisetum *or* Lycopodium. The gap between the Bryophytes and Vascular Cryptogams would thus be bridged over by a hypothesis based upon analogy. I am fully aware how open such a hypothesis is to criticism, but I think that it is better, after careful and widely extended comparative observation, to put out the hypothesis than to be content with no clear hypothesis at all. More especially is this so in the present case, where no intermediate types are at present known to exist, and where we have no special reason to expect that such types will be discovered.

The above description makes it evident that a revision of terminology of spore-bearing members will be necessary; if the spore-bearing member of the Ophioglossaceæ be homologous with the sporangium of *Lycopodium*, it is obviously undesirable to call it a "fertile frond." It could not, however, be termed a sporangium without violence to the meaning of this word. I have not yet arrived at a conclusion on such points as these, and shall defer the definition of the terms to be used until the memoir, of the contents of which this is a short and partial preliminary statement, shall be ready for presentation to the Society.

In conclusion, no reference has been made in this statement to the higher plants. Clearly the enunciation of new views with regard to the lower vascular plants must affect opinions as to the morphology of the higher. The interpretation of these will ultimately have to depend upon conclusions drawn from the study of the lower types. Hitherto it has been the practice to read the morphology of the lower forms in terms of the morphology of the higher; the converse will have to be ultimately adopted. Nevertheless, I have abstained at present from touching upon such questions as these, partly because such discussions would obscure the present issues, partly because the time has hardly yet come for any general statement.

The Society adjourned over the Christmas Recess to Thursday, January 14, 1892.

Presents, December 17, 1891.

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Photograph of Dr. Benjamin Franklin, F.R.S., from the Painting in the possession of the Royal Society. Mr. Hyatt.

"On the Demonstration of the Presence of Iron in Chromatin by Micro-Chemical Methods." By A. B. MACALLUM, M.B., Ph.D., Lecturer in Physiology in the University of Toronto. Communicated by Dr. H. NEWELL MARTIN, F.R.S. Received April 23,—Read April 30, 1891. Revised August, 1891.

The investigation, some of the results of which are to be given in the present paper, was stimulated by the conclusions of studies which I carried on during the last five years, and by the observations of Bunge* and Zaleski† on the occurrence of iron-holding proteids in the food and in the liver. The conclusions which I drew from my studies were:—

(1.) That in Amphibia the hæmoglobin is derived from the very abundant chromatin of the hæmatoblasts.‡

(2.) That in the maturing ova of Amphibia the abundant chromatin of the nucleus diffuses out from the latter, and, as the diffusion commences and progresses, the yolk spherules appear and increase in size. In other words, chromatin constitutes a large part of each yolk spherule.

(3.) That there is a transference of chromatin from the maternal tissues to the fœtal villi in the placenta of the Cat, and that this chromatin is carried from the central portions of the villi by the amœboid cells of the latter onwards towards the fœtus.

From the first conclusion it follows that the chromatin of hæmatoblasts contains iron. A consideration of the remaining conclusions points, however, to a wider distribution of iron. Bunge found in the yolk of the hen's egg an iron-holding nuclein, which, from its

* "Ueber die Assimilation des Eisens," 'Zeit. f. Physiol. Chemie,' vol. 9, p. 49.

† "Studien über die Leber.—1. Eisengehalt der Leber," 'Zeit. f. Physiol. Chemie,' vol. 10, p. 453.

‡ The observations on this subject are described in a paper which is to appear in the forthcoming number of the 'Transactions of the Canadian Institute.'

supposed connexion with hæmatopoiesis in the developing chick, he named hæmatogen. In the amphibian ova also the iron is united with a proteid fixed in the yolk spherules, and as this proteid has all the characters of a nuclein, it is, therefore, in all probability, the same compound as that described by Bunge. This nuclein is, apparently, none other than the chromatin diffused from the nucleus of the maturing ovum. If so, it is possible that the original unmodified nuclear chromatin of the ovum contains iron. Again, the fact, that the lower Vertebrata receive the iron which is present in them during larval life in combination with a nuclein, points to the occurrence during the embryonic period in Mammalia of the same or a similar endowment with an iron-holding nuclein. There is, in the Cat, as stated already, a transference of chromatin from the maternal to the fœtal tissues. Judging from all the aspects of these questions it was inferred also that, in the transference of chromatin to the embryo, the latter receives all the iron which it requires.

These points, taken in connexion with the fact that Zaleski isolated from the liver cells of various animals a nuclein in which iron is firmly bound, led me to the generalisation that the chromatin of every cell contains iron as a necessary constituent of itself. I determined to test the accuracy of this generalisation. Under my directions, Mr. R. R. Bensley isolated, from lamb's testicles and the calf's thymus, quantities of nuclein, which, when carefully purified, gave not the slightest iron reaction, but in the ash of which there was abundant evidence of the presence of the metal.*

Although care was taken to obtain the nuclein free from hæmatin, yet I was not certain that we did obtain it absolutely in a pure condition. It is well known that fixed or dead chromatin readily absorbs dyes, and, as it is extremely probable that this power is exercised on other substances, it may well be believed that

* In the preparation of nuclein from these sources we proceeded as follows:—The organs, freed from connective tissue structures, were finely minced and rubbed up in a glass mortar with a small quantity of a 2 per cent. solution of hydrochloric acid. To this fluid, after having been strained through moderately fine muslin, to remove small portions of fibrous tissue, more of the weak solution of the acid was added, with a small quantity of a glycerine extract of the mucosa of the cardiac portion of the pig's stomach. The fluid so prepared was kept at a temperature of 35° C. To the undigested residue left each day fresh quantities of the hydrochloric acid solution and of the glycerine extract were added, until the peptone reaction was no longer obtainable from it. It was next washed with alcohol, then carefully with ether, to remove all the fat, and submitted to the action of strong ammonia for twenty-four hours. A portion was thus dissolved, and the solution, freed from the insoluble part by filtration through iron-free paper, was next treated with three times its volume of alcohol. The precipitate from this was removed, again dissolved in ammonia, and reprecipitated with alcohol. The nuclein so obtained gave no iron to Bunge's fluid, nor did it give to alcohol acidified with sulphuric acid any hæmatin.

hæmatin is as readily absorbed and as tenaciously held by chromatin as are some dyes. Indeed, the accuracy of Zaleski's observations on *hepatin*, the iron-holding nuclein of the liver cells, may be open also to objection on this score. It is, to a certain extent, accepted that the liver is the organ which converts the hæmatin of disintegrated hæmoglobin into the bile and urine pigments. Now, the liver cells are probably the agents in this conversion, and the hæmatin which is held by them and their nuclei cannot, presumably, be removed by the washing out of the lobular capillaries with saline solutions.

Such being the difficulty, the solution appeared, at first sight, to lie in the preparation of nuclein from an animal from which hæmoglobin is absent. No opportunity presented itself for making such preparations of the substance in desired quantities, and it appeared to me that nuclein from such a source could not be free from the histo-hæmatins found by MacMunn to be present in all tissues, except those of the nervous system. With this difficulty before me, I felt compelled to relinquish that line of investigation, and to attempt another.

It occurred to me that it might be possible to demonstrate under the microscope the presence of iron in the chromatin of the cellular elements of such tissues as are almost wholly free from hæmatins, and in those tissues which are hardened so rapidly, as to prevent a transference from without of hæmoglobin or hæmatin into the cells. Such tissues are the cutaneous epithelium in Amphibia and Fishes, the cornea and cartilage in all Vertebrates. This demonstration I had often attempted without success, and I was forced to adopt the conclusion that, if iron is present in the intact nucleus, it is either so firmly bound in the molecules of chromatin that the ordinary reagents cannot attack it, or so small in quantity that its colour reactions are absent, even under the microscope. Bunge, however, in the case of hæmatogen, had shown that ammonium sulphide has the power of separating the iron; and Mr. Bensley and myself ascertained that this reagent has the same effect on isolated chromatin, when kept in a warm condition for a long time in contact with the substance, the iron separating as sulphide. I tried the effect of the reagent on tissues, and took, for example, pieces of the mesentery of *Necturus*, put them in a ground-stoppered bottle with ammonium sulphide, and kept the bottle in a warm oven for several days. At the end of that time, the pieces of tissue showed along the line of the blood-vessels a greenish-blue colour, while the remaining portions had a diffuse light-green colour. Under the microscope, the iron reaction appeared in the red blood cells and their nuclei, while the remaining cellular elements showed nothing further than a diffuse light-green colour in both cell and nucleus. I found, moreover, that the same results were obtained when the preparations were immersed in an

alcoholic solution of ammonium sulphide for twenty-four hours, and kept at the temperature of the laboratory. The pieces of tissue remained for two weeks or more in the warm ammonium sulphide, but no colour reactions appeared in the chromatin of the cells. Not satisfied with the conditions under which the experiments were carried on, I adopted other methods. It occurred to me that, if I kept a few cells for a long time completely separated from one another under a cover-glass, on a slide and surrounded with ammonium sulphide, the reaction might come out. To prevent the evaporation of the ammonium sulphide, I luted the edges of the cover to the slide with various luting compounds, only to find that, in some way or other, the preparation spoiled after a day or two. The luting method being useless, I employed another device. Glycerine, when kept in contact with ammonium sulphide at a moderately high temperature for some time, does not affect the latter reagent, and is itself unaffected. I teased out with clean goose-quill points on a slide a small piece of the testicle of *Necturus*, hardened in 70 per cent. alcohol, added a drop of freshly-prepared ammonium sulphide, put on a cover-glass, allowed a drop of glycerine to run in from its edge, and then placed the slide in a warm oven with a constant temperature of 60° C. Here it was allowed to lie for three days, at the end of which time I examined it under the microscope, and found that, in addition to that reaction distinctly present in the cell body and nucleus of each red corpuscle, there was one apparent in the nuclei of a group of testicular cells at one spot in the preparation. The slide was replaced in the warm oven, and the daily examination of it showed that, accompanying the increase in the number of cells presenting the iron reaction, there was an increase in the depth of the colour in those nuclei first affected, until, at the end of twenty days, the great majority of the testicular nuclei under a cover-glass, 16 mm. square, manifested a colour varying from light green or greenish-blue to dark-green or black. Under a high-power objective the colour was found confined to the chromatin nodules and nuclear network. After three weeks the nuclei adjacent to the edges of the cover-glass began to lose their stained appearance, until, finally, the chromatin possessed only a rusty appearance due to the formation of ferric oxide, for, when a mixture of hydrochloric acid and potassic ferrocyanide was allowed to run under the cover, the rust-coloured nuclei immediately assumed a deep azure-blue colour. That it was the chromatin alone in such cells which presented the reaction with ammonium sulphide was abundantly shown in the karyokinetic figures present in the same preparation. The achromatin and cell substance were unaffected.

Encouraged by the success of this experiment, I made a number of preparations from the other organs of *Necturus*, hardened also in 70 per cent. alcohol. Nearly all of these were successful, but the time

required for the production of the reaction varied greatly. A number of conditions seem to assist in, or retard, and even prevent the success of the experiment; but what are all the favourable conditions I do not know as yet. It is certain, however, that there is a proper proportion of glycerine and ammonium sulphide in the mixture to be added, and I am at present endeavouring to determine what that proportion is. It is also ascertained that the nucleus must be surrounded on all sides by the mixture, otherwise it very rarely shows the reaction. Where the cell body is large, as, for example, in the semi-matured ovarian ova of *Necturus*, the reaction has not yet appeared; while in those very small ova, in which the nucleus, rich in chromatin, forms by far the greater part of the cell, the reaction appears as readily as in the testicular nuclei. Again, the preparation may advance in the reaction up to a certain point, showing the majority of its nuclei possessing a light-green, greenish-blue, or slate colour; when a change occurs, the rust-tint replacing these colours in the chromatin. So far as my experience goes, this happens when too little sulphide has been mixed with the glycerine, or when the sulphide used is beginning to turn deep yellow, or is old.

The deep-yellow sulphide gives no reaction with nuclei, even after eight weeks, while the most active is the freshly prepared reagent. This seems to indicate that the process, by which the iron is set free from the chromatin, is essentially a reducing one, the yellow sulphide having much less reducing power than the colourless, or nearly colourless, reagent. In this way we can explain why the nucleus must be completely surrounded by the reagent, for the reducing capacity of the latter is limited, and a large quantity of it must be concentrated in its action on some particularly small object. When, however, the cellular elements are in a mass, not even the peripherally placed nuclei are affected in the manner described, but they react when they are teased out and separated.

In some tissues there is more or less of albuminate iron or of a deposit of inorganic iron compounds. In such a case the addition of ammonium sulphide gives a reaction immediately. The presence of such compounds sometimes offers a difficulty, especially if they happen to occur in the nuclei. I guarded against confusion in all such cases by submitting thin sections of such tissues made with the free hand to the action of a large quantity of Bunge's fluid for eight to ten hours.* Such sections also, if made from alcohol-hardened tissues with a clean steel knife, covered with absolute alcohol, are not in the

* This fluid consists of ninety volumes of 96 per cent. alcohol and ten volumes of hydrochloric acid, 25 per cent. strength. It, according to Bunge, extracts all inorganic iron compounds and albuminate iron from egg-yolk, and I have found that it removes all traces of such from sections of the spleen, liver, and kidney, which react immediately on the application of acid ferrocyanide solutions.

slightest degree impregnated with iron from the instrument. Zaleski,* has also found that, in sections of the liver made in the usual way, there is nothing in the distribution and quantity of iron present, different from what is observed in sections of the same piece of tissue prepared with a glass knife.

Having ascertained the conditions, generally, under which the presence of iron is successfully demonstrated in chromatin, I tried once more the experiments with those tissues which can readily be obtained free, at least, from hæmoglobin. I found that the reaction came out definitely and distinctly with the chromatin of the corneal and cutaneous epithelium, and in the nuclei of the cartilage cells of *Necturus.* Here, as in the other instances, the time required for the production of the reaction was found to vary greatly, and two preparations from the same organ, *e.g.*, the cornea, presented differences in this respect.

I have succeeded in obtaining the iron reaction in the chromatin of the cells of the following organs and tissues of *Necturus*:—testicle, ovary, gastric and intestinal epithelium, gastric and intestinal glands, pancreas, liver, kidney, cartilage of tongue and shoulder girdle, the cutaneous epithelium, the mesenteric endothelium and connective tissue, and the muscularis of the intestine. I found also the iron reaction in the chromatin of red, white, and fusiform cells of the blood.

From two human placentæ of about five and seven weeks respectively, thoroughly freed from hæmoglobin before hardening, and having the appearance of bleached linen, I removed portions, which I washed carefully with a mixture of alcohol and sulphuric acid to deprive them of traces of hæmatin, and subjected them to the action of warm ammonium sulphide on the slide. The nuclei of all the isolated epithelial cells of such gave a beautiful and intense iron reaction after ten days. Indeed, certain parts of the preparations reminded one of the iodine-green nuclear stain, but after being three weeks in the warm oven, the colour became greenish-black. The nuclei of the hæmatoblasts in the villi give the iron reaction in twenty-four hours, and about two to four days are required to show that the masses situated in the eosinophilous amœbocytes scattered through the connective tissue of the villi also contain iron.

The iron reaction was obtained at the end of two weeks in the nuclei of the epidermal cells of a fœtal kitten removed at about half term from the recently killed cat in a way to prevent the absorption of, or contamination with, hæmoglobin.

Treatment of sections of the placenta of the cat with warm ammonium sulphide also shows, as I expected, that with the passage of chromatin from the maternal to the fœtal tissues, there is also a

* *Loc. cit.*, p. 483.

transference of iron to the embryo. At the base of the placental mucosa, there are glands whose cellular elements pass through a history not unlike in some respects that of the mammary gland. They proliferate, enlarge in size, apparently extrude particles like fat into the lumen of the gland, and then they undergo chromatolysis. The masses of chromatin set free can be readily recognised in the *débris*. In some cases the upper wall of the gland is broken through by the extremity of a villus, whose elongated epithelial cells now stretch amœba-like towards the *débris*, particles of which they invaginate, and among these, chromatin granules. The latter finally reach the centre of the villus, and, with the chromatin obtained from disintegrated maternal endothelial cells, form there a more or less compact column of chromatin. When the embryo measures 25 mm. in length, the amount of chromatin is small, but in considerably later stages it is so abundant that, in stained sections of 30 μ in thickness, the masses formed of it can be seen with the naked eye. In the younger placentæ the chromatin gives, with warm ammonium sulphide, the reaction at the end of twenty-four hours, but the presence of iron is not indicated by hydrochloric acid and potassic ferrocyanide. In the older placentæ, the acid and ferrocyanide mixture gives the iron reaction with the chromatin masses at once, as does also the ammonium sulphide. Now, the chromatin granules in the *débris* of the glands at the base of the placentæ do not in any case give a reaction with the acid mixture, while with warm ammonium sulphide they show the presence of iron after two days. From this it is to be concluded that the chromatin of disintegrated cells manifests more and more readily as time goes on the reaction with ammonium sulphide, and my experiments with this reagent on degenerating cells in other organs confirm this conclusion.

Now in such sections of the placentæ, the chromatin of none of the ordinary cells gives the reaction, even if the sections are kept for weeks in contact with warm ammonium sulphide, either in a bottle or under a covered glass. If the cells are teased out from one another, so that they lie free and separate under the cover, the reaction is obtained in each in about ten days, and it is as distinct as in the chromatin granules in the glandular *débris*, or as in the masses in the central parts of the villi.

I have also obtained the iron reaction in the chromatin in the intestinal cells and the maturing ova of *Oniscus*, in the nuclei of the maturing ova, and of the spermatozoids of *Ascaris mystax*, and in the smaller cells of the larvæ of a species of *Chironomus* found on the stones in running water in the neighbourhood of Toronto in winter. The cells of the salivary gland of the latter animal are too large to give the reaction readily, and, as I write, it has only now begun to appear in the nucleolus in which the chromatin filament terminates.

In the smaller cells, the reaction seems to differentiate between the chromatin and linin parts of the filament.

I may state also, that by this method the iron-holding compound of the muscle-fibre in *Oniscus* is found to be confined to its dim bands.

I have repeatedly employed the hydrochloric acid and potassic ferricyanide mixture to show that the green or greenish-black compound resulting in the nuclei from the action of ammonium sulphide is ferrous sulphide. For this purpose, the nuclei, which are rich in chromatin, *e.g.*, those of the testicular cells and of the immature ovarian ova in *Necturus*, are the best. Usually I washed out the glycerine and sulphide mixture from under the cover-glass by the addition of a large drop of a mixture of equal parts of glycerine and water, and, after some hours, when it had run under the cover, a strip of paper touching the opposite side drained away a portion of this. A repetition of this process several times left very little sulphide, and, very often, few cells under the cover. The addition now of a drop of a mixture of weak hydrochloric acid, and of freshly prepared potassic ferricyanide led to the formation in the previously green or greenish-black nuclei of a deep azure-blue colour, strictly limited to the parts originally affected with the ammonium sulphide. This reaction is sharp, and comes out almost immediately, whereas when hydrochloric acid and potassic ferrocyanide are used, the blue reaction comes out in about half-an-hour, and the colour seems to diffuse through the nucleus and sometimes into the cell. The acid and ferricyanide mixture I have also employed successfully on the nuclei of the cutaneous epithelium, and of the hepatic, gastric, intestinal, and pancreatic cells of *Necturus*, which had previously reacted with ammonium sulphide. In no case was it found that the immediate application of the acid reagent mentioned, or of acid ferrocyanide solution, gave the slightest reaction with those species of nuclei which required a more or less lengthy contact with ammonium sulphide in order to develop the iron reaction. In every particular instance referred to, the latter reagent had to be employed to decompose the chromatin, and set free its iron as sulphide, and the acid mixtures then, and then only, gave a deep azure-blue colour.

Now it might be urged that this iron reaction is due to hæmatin or an allied iron compound. The observations which I now proceed to detail will, I think, completely meet this objection.

Believing that if iron enters into the composition of the chromatin of the animal cell, it must be also present in that of the vegetable cell, I asked Mr. Bensley to employ my method in studying the distribution of iron in the latter. His investigations, so far as they have gone, have confirmed mine, since he has found that the chromatin of the pollen cells of *Dianthus*, *Cucurbita*, *Narcissus*, and of the

cells of pollen sacs of *Hyacinthus*, all fixed in alcohol, give, with several days' application of warm ammonium sulphide, under the cover-glass, a greenish-blue or a dark green reaction.

We have observed that the chromatin of the karyokinetic figures in the pollen grains of *Cucurbita* shows an intense coloration with ammonium sulphide. It has, moreover, been found that there is here, as shown by the application of staining reagents (Ehrlich's hæmatoxylin and Czokor's alum cochineal), a diffusion of the chromatin from one of the two nuclei of the maturing pollen grain into the pollen cell, and this diffusion continues till, finally, there is, comparatively, little chromatin left in the shrunken nucleus.* While the diffusion is taking place, the chromatin is more abundant in the immediate neighbourhood of the nucleus. Now in such maturing pollen grains, hardened in alcohol, there is produced by ammonium sulphide after several days' stay in the warm oven, an iron reaction, corresponding in intensity and distribution with the colour produced by the staining reagents, diffuse in the nucleus, strongly marked in its neighbourhood, and slightly at the periphery of the cell. As the maturation of the pollen grain progresses, the iron reaction is more readily obtainable, and, when the maturation is apparently complete, the pollen cell gives, with freshly prepared ammonium sulphide, in a few hours a light green reaction which becomes but a shade deeper after several days' stay in the warm oven.

Mr. Bensley has also been able to determine with the ferrocyanide mixture the passage of iron salt along the bast portion of the fibro-vascular bundles in the ovary after the opening of the flower, and he has traced these iron salts in sections of the ovary through the raphe of the ovules up to the boundary line of the latter. Beyond this point the iron salts, if they advance, become hidden or disposed of in such a way that they no longer give reactions with acid solutions of potassic ferrocyanide or ferricyanide. Nor do sections of the ovules show any reaction with warm ammonium sulphide, either under a cover-glass or in the bottle. Taught by the experiments on animal cells, I teased out with goose-quill points sections of the ovules in ammonium sulphide and glycerine on the slide, so far as to isolate the various parts of the ovule, and, after keeping the preparation in the warm oven for three days, the nuclei of nearly all the separate and individual cells showed a dark green reaction, which was due to the presence of iron, as the application of a mixture of dilute hydrochloric acid and potassic ferricyanide proved. I have been able in this way to deter-

* A similar diffusion of nuclear substance into the pollen cell takes place, according to Strasburger, in those Angiosperms in which each pollen grain develops numerous pollen tubes ('Sitzungsber. der Niederrhein. Gesell. für Natur- und Heilkunde,' December, 1882, referred to in Just's 'Botanische Jahresberichte' for that year).

mine the presence of iron in the chromatin of a large number of vegetable cells.

I asked Mr. J. J. MacKenzie to undertake the study of the distribution of iron in fungi and algæ, and very encouraging have been, so far, the results of his examination. He has found, for example, that in the gonidia of *Cystopus candidus*, hardened in alcohol, the application of warm ammonium sulphide and glycerine on the slide for eight days brings out the presence of four or more blue-green, round bodies, measuring 1·6 μ in diameter, and corresponding to the nuclei of the zoogonidia, the rest of the protoplasm of the gonidia remaining absolutely uncoloured. The coloured parts gave one the impression as if the gonidia had been given a purely nuclear stain with iodine-green. Mr. MacKenzie has also observed interesting results following the employment of ammonium sulphide on some blue-green algæ, which indicate that here also there is a substance like chromatin in firm combination with iron.

I think that enough has been advanced to show that my view, that the chromatin of every cell, animal and vegetable, is an iron-holding compound, is one which is now capable of proof. I cannot assert that it is proved as yet, since I am aware that that can only be done after an extensive series of observations made and careful work performed. Mr. MacKenzie, Mr. Bensley, and myself are continuing the investigations outlined, and we hope before long to be in a position to bring forward an abundance of interesting facts, which are now accumulating, and of which those given here are examples.

I forego any speculation as to the bearings which my observations may have on our knowledge of animal and vegetable metabolism. I content myself here with referring briefly to the condition in which the iron is present in the chromatin. As stated already, I have found that the hæmoglobin of Amphibia is formed from the chromatin of the hæmatoblasts. This would seem to indicate that the iron is attached in the chromatin molecule to an atom-group somewhat like that of hæmatin. As the oxygen-carrying property of hæmoglobin is generally attributed to the presence of the iron in it, we may ask ourselves whether the chemical processes in the chromatin of the living cell are due to a constant alternation of the oxidised and reduced conditions of the iron in the chromatin molecule. As hæmoglobin results from degeneration or disintegration of chromatin, we would naturally expect to find in it one or other condition specially prominent. The more stable condition is that of oxidation. It is possible that in living chromatin the conditions are more readily interchangeable, and that therein lies a basis for a theory of those chemical processes of the cell which are grouped under the term "vital."

"On the Bases (Organic) in the Juice of Flesh. Part I." By George Stillingfleet Johnson, M.R.C.S., F.C.S., F.I.C. Communicated by Professor G. Johnson, F.R.S. Received April 28,—Read May 28, 1891.

The object which I have kept in view in the research, the first results of which are described in this paper, has been to ascertain as far as possible what substances are really present in the watery extract of fresh muscle, and which of the substances commonly described as being present in it are really due to changes taking place in the flesh during the processes of analysis—in short which of the substances obtained from flesh are *educts*, and which are mere *products* therefrom.

And, in this investigation, I have been on my guard against two great sources of error, viz.:—

1st. Changes produced in the ingredients of the muscle-substance by chemical agents and chemical or physical forces generally; and

2ndly. Changes brought about by bacterial action.

This latter source of error seems to me of extreme importance, since it is now well known that profound changes are effected in the composition of such susceptible bodies as flesh through the agency of bacteria, long before those grosser effects are produced which lead to the development of fœtid gases, &c., and which are commonly described as "putrefaction."

Accordingly, I shall describe my experiments in the order in which they are carried out, viz.:—

1st. Preliminary experiments, which are especially designed to exclude the first source of error; and

2ndly. Experiments designed so as to exclude, as far as possible, both sources of error.

1*st*. *Preliminary Experiments.*

It is well known that when kreatinin is kept in a watery solution, whose reaction is alkaline, at the boiling temperature for a length of time, the base is gradually converted into kreatine by assimilating to itself the elements of water.

Now, in Liebig's process for preparing kreatine from flesh, the radical of phosphoric acid is precipitated from the watery extract of the muscle-substance by addition of baryta-water, so long as any precipitate occurs; one result of which treatment is that the liquid becomes strongly alkaline. It has, therefore, been suggested that during the concentration of the alkaline solution any kreatinin origi-

nally present in the flesh extract would undergo conversion into kreatine, and consequently that the kreatine which is ultimately obtained may have resulted either partly or entirely from a conversion of kreatinin into kreatine, in short that the kreatine is a mere *product* from the flesh, not a true *educt*. My first experiment was designed to test this theory.

Experiment I.—70 lb. of fresh butcher's beef was finely cut up by a sausage machine, after being freed as far as possible from fat and bone, and thoroughly incorporated with water by hand-kneading. By means of a specially designed screw-press, which was made for me by Messrs. Farrow and Jackson, the aqueous extract was removed from the fibre. This process was repeated with fresh additions of water, until four extracts were obtained, each extract being separately examined.

The albuminoid substances were separated from all the extracts by heat (about 80°C.), most of the colouring matter being carried down by the coagulated albumen. The filtrates were then concentrated by evaporation over steam, until a scum began to form upon the surface, after which the further concentration was effected *in vacuo* over sulphuric acid, by means of a Carré's freezing machine. No chemical agent was added to any of the extracts at any time, until the concentration was complete.

In each case the residue obtained by concentrating these extracts was partly crystalline and partly gelatinous.

Complete separation of the crystalline from the amorphous matter was readily effected by means of dilute alcohol, which left the crystalline matter undissolved.

Finally, by fractional crystallisation from water, the crystalline matter was resolved into an organic and an inorganic substance.

The organic crystals were kreatine.

The inorganic salt was acid potassium phosphate, KH_2PO_4.

The results of this experiment prove—

1st, that kreatine may be obtained from the watery extract of flesh whose natural acidity has not been chemically neutralised, so that hydrolytic conversion of kreatinin into kreatine is most improbable; and,

2ndly, that the presence of phosphates in solution does not interfere—as has been stated to be the case by some observers—with the crystallisation of kreatine from extract of flesh.

Inasmuch as I am not aware that acid potassium phosphate has been actually obtained in crystals from the watery extract of muscle hitherto, I have thought it advisable to give analytical results which prove the identity of the substance.

The salt is much more soluble in water than the kreatine with which it is associated, so that the separation of the two substances is

easy. It occurs in hard glistening anhydrous prisms usually radiating from a common centre. The aqueous solution of these crystals is acid to litmus and gives a yellow precipitate on addition of silver nitrate solution. This precipitate (Ag_3PO_4) is much increased on carefully neutralising the liquid with ammonia.

When heated the crystals lose water and fuse. The fused mass dissolves slowly in water to a *neutral* solution, which gives a white flocculent precipitate with silver nitrate ($AgPO_3$).

1·349 grams of the crystals lost 0·1835 gram of H_2O on ignition, corresponding with 13·602 per cent of the original weight.

According to the equation $KH_2PO_4 = KPO_3 + H_2O$, the crystals would theoretically lose 13·22 per cent.

0·985 gram of the crystals gave 2·999 grams of Ag_3PO_4, corresponding with 0·680 gram PO_4, *i.e.*, 69·03 per cent. PO_4.

The filtrate from the Ag_3PO_4 was freed from silver by HCl, and the K in the filtrate converted into K_2SO_4 by H_2SO_4, evaporation, &c.

The weight of K_2SO_4, obtained from 0·985 gram of the KH_2PO_4, was 0·6652 gram, which corresponds with 0·2986 gram of potassium, or 30·31 per cent. K.

The *hydrogen* in the salt was determined by titration with limewater of known strength. 0·112 gram of the salt was neutralised by 0·05739285 gram of $Ca(HO)_2$.

According to the equation $3KH_2PO_4 + 3Ca(HO)_2 = Ca_3(PO_4)_2 + K_3PO_4 + 6H_2O$, this result indicates 0·00155 gram of hydrogen, or 1·38 per cent. of hydrogen.

These results agree with the formula KH_2PO_4.

	Required for KH_2PO_4.	Found.
Loss of H_2O on ignition	13·22	13·602
K	28·72	30·31
H	1·47	1·38
PO_4	69·81	69·03
	100·00	100·72

The preparation of pure acid potassium phosphate from the watery extract of flesh is especially interesting as showing how a mere product may be taken for an educt.

It is well known that Liebig has described *potassium chloride* as a constituent of fresh muscle substance; and no doubt many observers who have followed out his process for the preparation of kreatine have obtained cubical crystals of potassium chloride on treating the mother liquors from the kreatine with alcohol. I have myself never

failed to obtain this salt after the use of baryta-water, as directed by Liebig.

But the action of baryta-water on the watery solution of KH_2PO_4 would be represented thus—

$$2KH_2PO_4+3Ba(HO)_2 = Ba_3(PO_4)_2+2KHO+4H_2O.$$

And the potassic hydrate thus produced would of course decompose the hydrochlorides of any organic bases present in the solution, forming potassium chloride and liberating the bases. The KCl, therefore, would be a *product*, not *an educt, from the flesh.*

Experiment II.—This was a preliminary experiment made with 50 lb. of butcher's beef, in order to ascertain the action of *mercuric chloride* in aqueous solution. Briefly summed up, the results were as follows:—

The addition of a sufficient quantity of mercuric chloride solution to the watery extract of fresh beef causes complete and instantaneous precipitation of the albuminoid constituents and of the whole of the colouring matter.

The filtrate from the coloured albuminous precipitate deposits, on standing, a spherical mercury salt, isomorphous with the spherical mercury salt of urinary kreatinin described by me in the 'Roy. Soc. Proc.,' vol. 43, pp. 493—534.

This spherical mercury salt yields a kreatinin isomorphous with the tabular kreatinin of urine, when subjected to similar treatment to that described in my paper quoted above.

Encouraged by these results, I determined to apply the mercuric chloride method, which I had formerly made use of in examining the kreatinin of urine, to the examination of the bases in the juice of flesh.

2nd. *Experiments Designed to Exclude Errors due to Bacterial Action, as well as those due to Chemical and Physical Action.*

Before describing these experiments, I will briefly indicate some of the special advantages possessed by what I have called the "mercuric chloride method" for the examination of organic bases.

The method itself, as applied to the examination of urine, is fully described in my paper quoted above. It enabled me to isolate from human urine what I believe to be the excrementitious kreatinin which has always been secreted by the human kidney, but whose properties I was the first to describe.

Perhaps the claims of my method to excellence in preparing an *educt* will be best appreciated by comparison with other methods, as in the following table.

Methods for Isolation of Kreatinin from Urine.

I. Heintz and Pettenkofer.	II. Liebig.	III. Maly.	IV. The Author.
Fresh urine neutralised with sodium carbonate, evaporated to a syrup. Syrup exhausted with alcohol, and alcoholic zinc chloride added.	Fresh urine neutralised with milk of lime. Calcium chloride added to complete precipitation. Filtrate evaporated till the salts crystallise out. Zinc chloride added to the liquor. Kreatinin zinc chloride dissolved in boiling water and treated with lead hydrate at the boiling temperature. Filtrate evaporated.	Urine evaporated to $\frac{1}{3}$rd of its original bulk. Lead acetate added. Filtrate freed from lead by H_2S. Filtrate neutralised by sodic carbonate and precipitated by mercuric chloride. Precipitate suspended in water, and decomposed by H_2S. Filtrate evaporated. Residue recrystallised from alcohol.	Fresh urine + $\frac{1}{20}$th of its volume of cold saturated solution of sodic acetate, + $\frac{1}{4}$th of its volume of cold saturated solution of mercuric chloride. Filter immediately. Collect the precipitate which forms in the filtrate in 48 hours. Decompose the Hg salt by H_2S under water. Treat filtrate with $Pb(HO)_2$ at ordinary temperature. Evaporate filtrate *in vacuo* over H_2SO_4.
	Product, kreatinin mixed with kreatine.	Product, kreatinin hydrochloride.	Product, efflorescent urinary kreatinin.

The following is a brief summary of the advantages possessed by the mercuric chloride method, which render it peculiarly applicable to the case of such an easily changed substance as fresh muscle:—

1. The germicidal action of mercuric chloride is so powerful that, if added in sufficient quantity, bacterial action is rendered impossible.
2. It removes from solution *at once* the more putrescible constituents of the liquid (albuminoid matters).
3. It precipitates kreatiuin *gradually*, so as to allow of separation of that base as mercury salt, and of subsequent isolation of the base itself, without any application of heat.
4. By removing from solution the putrescible substances, it favours the isolation of any bases, &c., which it does not precipitate, inasmuch as these substances are protected by it from any danger of alteration by bacterial action.

Having thus indicated my reasons for adopting the mercuric

chloride method in examining the bases in the juice of flesh, I will now describe the details of my experiments, in which both bacterial action, and change due to chemical agents have been avoided as far as possible.

The two experiments described above were made with ordinary butcher's meat; and, although the substance was not of course grossly putrid, I had no precise knowledge of the date of the death of the animals which supplied it, and consequently no knowledge of the duration of exposure of the flesh to aërial, *i.e.*, to bacterial, influences.

I was enabled to overcome this difficulty, and to avoid this uncertainty, by the kind help of Professor G. T. Brown, who volunteered to obtain for me a healthy animal at the Royal Veterinary College, and to allow me to commence my experiments in the chemical laboratory at that institution.

Experiment, commenced at the Royal Veterinary College.

A healthy cow was killed at 10.45 A.M. on Thursday, January 3, 1889.

I am indebted to Professor G. T. Brown for the following account of the mode in which the animal was killed:—"The cow was killed by an expert slaughterman from the Metropolitan Abattoirs at Islington. As your object was to free the system from blood as quickly and completely as possible, the animal was rendered unconscious by a single blow from the poleaxe, and instantly the large vessels emerging from the front of the chest were divided. The death of the cow was almost instantaneous."

Professor Brown examined the internal organs, and assured me that he had found no trace of organic disease in any of them.

The flesh was removed at once from the carcass, and brought to me whilst still warm.

After chopping some of it finely, I endeavoured to express juice from it with my specially constructed screw press, but not a drop could be obtained. It, therefore, became necessary to add water to the macerated muscle.

30 lb. (= 13·62 kilograms) of the finely-chopped flesh was minced thoroughly by kneading with the fingers with 5 litres (5 kilograms) of water. This was done at 12 noon January 3. At 3 P.M. on the afternoon of the same day the expression of the juice was commenced, and was completed at 5.30 P.M., *i.e.*, rather *less than seven hours after the death of the animal.* In all 2500 c.c. of a red liquid was squeezed out, mixed with 3750 c.c. of a solution of mercuric chloride, saturated at 15° C., and filtered immediately. Both precipitate and filtrate were of course preserved. From this time onwards the substances obtained

from these 30 lbs. of beef were free from all danger of change by bacterial action; and the source from which the extract was made had been exposed to bacterial action for only seven hours. This extract will be spoken of as "portion A."

The muscle fibre from which the above extract was taken was left in a porcelain bath all night (the weather was very cold at the time), and no more water was added to it.

On Friday, January 4, this fibre was again put through the press, and, although as much as possible had been squeezed out of it on the previous day, and no more water had been added, an additional 3750 c.c. of a red liquid was obtained.

The total volume of liquid obtained from these 30 lbs. of flesh amounted, therefore, to 6250 c.c., whilst the volume of water added was 5000 c.c. Therefore, 1250 c.c. of actual juice must have been expressed from 13·62 kilograms of flesh, *i.e.*, the juice obtained was about $\frac{1}{11}$th of the weight of the flesh taken.

This second portion of juice was not mixed with portion A, but 2250 c.c. of the mercuric chloride solution was added to it at 1 P.M. on Friday afternoon, and it was filtered at once. The precipitate and filtrate were preserved and labelled "Portion B." The utmost exposure to bacterial action undergone by portion B was twenty-six hours.

So far there is evidence that one result of bacterial action is to render the muscle substance more fluid.

The following four portions of flesh from the same cow were extracted with water, and the extracts mixed together in one large vessel with mercuric chloride solution:—

(1.) 22 lb. 8 oz. + 4 litres of water at 4 P.M. on Thursday January 3, and left all night.

Expressed 5220 c.c. of red liquid (of which 1220 c.c., or about $\frac{1}{10}$th of the flesh, must have been juice) at 3 P.M. on Friday, and added 2000 c.c. of $HgCl_2$ solution.

(2.) 18 lb. 12 oz. (= 8·576 kilograms) of flesh + 3 litres of water at 1.30 P.M. on Friday, January 4.

Expressed 3700 c.c. of red liquid (of which 700 c.c., or $\frac{1}{12}$th of weight of the flesh, must have been juice) at 6 P.M. on Friday, January 4, and added 2000 c.c. of $HgCl_2$ solution.

(3.) 22 lb. 8 oz. (= 10·215 kilograms of flesh) + 3 litres of water at 4 P.M. Friday.

3000 c.c. of red liquid squeezed out at 8 P.M. Friday, and 2000 c.c. of $HgCl_2$ added.

(4.) 22 lb. 8 oz. of flesh + 3300 c.c. of water.

Expressed 3100 c.c. of red liquid at 9 P.M. on Friday, January 4, and mixed with 2000 c.c. of $HgCl_2$ solution.

The juice from these last four portions was mixed with the mer-

curic chloride in one vessel, and this mixed extract will be described as "portion C." The entire weight of flesh contributing to portion C amounted to 86 lb. 4 oz. (= 39·22 kilograms), and the duration of exposure to bacterial action varied from 26 to 34 hours. After standing a week, the turbid liquid was filtered. Filtrate and precipitate both preserved, and labelled "Portion C." Altogether, then, I succeeded in working up 116 lb. 4 oz., or 52·84 kilos., of lean muscle-fibre from the cow.

In this paper I shall describe only the results obtained from the examination of the three *filtrates* after addition of mercuric chloride, viz., portions A (exposed 7 hours to air), B (exposed 26 hours to air), and C (exposed 34 hours at most).

My endeavour has been to treat these three filtrates in exactly the same way as far as possible, so as to avoid introducing any change in one of them by an agency to whose influence the others were not exposed. By these means I should feel justified in attributing any difference between the products obtained to bacterial action upon the flesh before the addition of the antiseptic mercuric chloride solution. One change took place in all the three solutions, viz., the very gradual separation of a white precipitate, which appeared granular macroscopically, and which consisted of minute spheres of perfect transparency when examined microscopically. In short, all the filtrates gradually deposited a spherical mercury salt isomorphous with the mercury salt of the kreatinin of urine.

These precipitates were not finally separated by filtration until the liquid had in each case ceased to deposit the spherical compound.

The spherical precipitate was separated from portion A in April, 1890, washed, dried at the ordinary temperature, and weighed. Its weight was 44·16 grams.

The spherical compound was separated from portion B in February, 1891. Its weight was 28·01 grams.

The spherical compound was separated from portion C in January, 1890. Its weight was 76·5 grams.

The three filtrates from these precipitates all remained perfectly clear on standing for not less than a week in each case, showing that the mercuric chloride still in solution had no further power to cause formation of insoluble compounds.

It is remarkable that the precipitates from A and B, which contain the extracts from 30 lb. of flesh, weigh together nearly as much as the entire precipitate from portion C, which is derived from 86 lb. 4 oz. of flesh. This lesser proportional weight of Hg precipitate from the portion C cannot be accounted for by the delay of a week in the filtration of the first precipitate by the mercuric chloride, because the separation of the spherical precipitate is so extremely slow in the case of the watery extract of flesh. The ex-

planation of this diminution in the weight of spherical mercury salt obtained from portion C must therefore be sought in the more prolonged exposure of the flesh from which this extract was obtained to bacterial influences.

The spherical mercury salts obtained as above were decomposed by H_2S under water, and the acid filtrates evaporated *in vacuo* over H_2SO_4 at the ordinary temperature. In each case crystals were obtained isomorphous with the hydrochloride of urinary kreatinin. These crystals when dissolved in water yielded acid solutions which became strongly alkaline when digested with pure lead hydrate at the ordinary temperature. On evaporating the alkaline filtrates *in vacuo* over sulphuric acid, the sarcous kreatinin formed in each case anhydrous crystals isomorphous with the tabular kreatinin of urine.

This sarcous kreatinin, therefore, differs from the urinary kreatinin in yielding anhydrous tables instead of efflorescent prisms when prepared without application of heat.

It appears, however, that the sarcous kreatinin may be rendered efflorescent by similar treatment to that which changes tabular urinary kreatinin into the efflorescent base, for the washings from the precipitate by lead hydrate in solution of hydrochloride of kreatinin from portion C were evaporated at 60° C., instead of at the ordinary temperature, and the acid solution was treated with $Pb(HO)_2$, filtered, and the alkaline filtrate also evaporated at 60° C. A number of long transparent needles formed, isomorphous with the efflorescent kreatinin of urine, and these needles became opaque when dry. On redissolving the efflorescent base in water and again evaporating, tabular kreatinin separated out.

A further comparison of the properties of this sarcous kreatinin reveals additional differences between this base and the kreatinin of urine.

In the table on p. 528, vol. 43, of the 'Proceedings,' I have laid stress upon the following points in comparing different kreatinins :—

Solubility in water and alcohol, properties of platinum and gold salts, and reduction of CuO, compared with glucose.

Accordingly I have especially examined the sarcous kreatinin, obtained as above described, with relation to these particulars.

(1.) *Solubility in Water of Sarcous Kreatinin.*

4·1995 grams of solution of sarcous kreatinin in water, saturated at 13·7° C., left, on evaporation, 0·3575 gram of kreatinin.

Therefore, 3·8420 grams of water dissolved 0·3575 gram of kreatinin.

Hence, 1 part by weight of kreatinin is dissolved by 10·74 parts by weight of water at 13·7° C.

(2.) *Solubility in Alcohol of Sarcous Kreatinin.*

14·9815 grams of solution of sarcous kreatinin in alcohol of sp. gr. 0·800, saturated at 13·7° C., left, on evaporation, 0·0305 gram of kreatinin.

Therefore, 14·9510 grams of alcohol dissolved 0·0305 gram of sarcous kreatinin at 13·7° C.

Hence, 1 part of kreatinin dissolves in 490·2 parts of alcohol at 13·7° C.

(3.) *Properties of the Platinum Salt of Sarcous Kreatinin.*

When sarcous kreatinin is dissolved in dilute hydrochloric acid, and a solution of platinic chloride added, an orange-coloured platinum salt separates out in crystals on evaporation over sulphuric acid. This platinum salt resembles that of urinary kreatinin in containing 2 mols. of water of crystallisation, which are expelled at 100° C., leaving the anhydrous salt as a lemon-yellow mass.

2·8577 grams of air-dried platinum salt of sarcous kreatinin lost 0·1567 gram of water at 100° C., becoming 2·7010 grams of anhydrous platinum salt.

These numbers correspond with a loss of 5·47 per cent. H_2O.

Required for $2(C_4H_7N_3O.HCl).PtCl_4.2H_2O$.	Found.
5·34 per cent. H_2O.	5·47 per cent. H_2O.

In calculating the percentage composition of the platinum salt of sarcous kreatinin, I have adopted the following atomic weights :—

$$C = 12, \quad H = 1, \quad N = 14, \quad O = 16, \quad Cl = 35{\cdot}5, \quad Pt = 194{\cdot}4.$$

0·5288 gram of platinum salt of sarcous kreatinin, dried at 100° C., left, after ignition, 0·1628 gram of platinum.

According to these numbers, the dry platinum salt contains 30·78 per cent. of platinum.

Required for $2(C_4H_7N_3O.HCl).PtCl_4$.	Found.
30·59 per cent. Pt.	30·78 per cent. Pt.

Determination of the Solubility in Water of the Platinum Salt of Sarcous Kreatinin.

3·942 grams of solution of the platinum salt of sarcous kreatinin in water, saturated at 15° C., left, on evaporation at 100° C., 0·167 gram of dry platinum salt.

According to these numbers, 22·6 parts by weight of water dissolve 1 part by weight of the platinum salt.

4. *Properties of the Gold Salt of Sarcous Kreatinin.*

When sarcous tabular kreatinin is dissolved in diluted hydrochloric acid, and the solution mixed with one of auric chloride, a splendid gold salt crystallises out, on evaporation over sulphuric acid, in glistening yellow plates, which are permanent in the air and lose no weight at 100° C.

0·1060 gram of the gold salt of sarcous kreatinin, having been dried at 100° C., left on ignition 0·0465 gram of gold, equivalent to 43·86 per cent. of Au.

Required for $C_4H_7N_3O.HCl.AuCl_3$.	Found.
43·46 per cent. Au.	43·86 per cent. Au.

In the above calculation, the following atomic weights were employed:—

$$H = 1, \quad C = 12, \quad N = 14, \quad O = 16, \quad Cl = 35{\cdot}5, \quad Au = 196{\cdot}8.$$

The gold salt of sarcous kreatinin differs from the gold salts of all the urinary kreatinins, described by me in June, 1887, in that it is completely dissolved by ether.

However, during evaporation of its ethereal solution, even at the ordinary temperature, the salt undergoes decomposition, and a mixture of auric chloride and the hydrochloride of sarcous kreatinin crystallises out.

5. *Reduction of Cupric Oxide in Boiling Alkaline Solution by Sarcous Kreatinin, compared with that of Glucose.*

0·1 gram of sarcous kreatinin was dissolved in 50 c.c. of water.

14·2 c.c. of this solution decolorised 40 c.c. of Pavy-Fehling solution, *i.e.*, 0·0284 gram of kreatinin reduces as much cupric oxide as 0·02 gram of glucose. Therefore 10 grams of $C_6H_{12}O_6$ are equivalent in reducing power to 14·2 grams of $C_4H_7N_3O$. Therefore 4 mols. of glucose = 9 mols. of sarcous kreatinin,

for $$10 : 14{\cdot}2 :: \underset{= C_6H_{12}O_6}{180} : 255{\cdot}6$$

and $$\underset{= 4 \times (C_6H_{12}O_6)180}{720} : \underset{= 9 \times 113(C_4H_7N_3O)}{1017} :: 180 : 254{\cdot}25.$$

The differences between the properties of the sarcous and urinary tabular kreatinins will be apparent at a glance when arranged in a table, as follows:—

Comparison between Sarcous and Urinary Tabular Kreatinins.

	Tabular kreatinin of urine.	Tabular kreatinin from urinary kreatine.	Sarcous kreatinin.
Solubility in water	1 in 10·78 at 17° C.	1 in 10·68 at 16·5° C.	1 in 10·74 at 13·7° C.
Solubility in alcohol	1 in 362 at 17° C.	1 in 324 at 18·5° C.	1 in 490·2 at 13·7° C.
Properties of platinum salt	Contains 2 mols. H_2O. Solubility 1 in 14·1 at 15° C.	Contains 2 mols. H_2O. Solubility 1 in 24·4 at 15° C.	Contains 2 mols. H_2O. Solubility 1 in 22·6 at 15° C.
Properties of gold salt	Unchanged by ether.	Decomposed by ether.	Soluble in ether. Decomposed on evaporation.
Reduction of CuO, compared with that of glucose	4 mols. $C_4H_7N_3O$ = 2 mols. glucose.	5 mols. $C_4H_7N_3O$ = 2 mols. glucose.	9 mols. $C_4H_7N_3O$ = 4 mols. glucose.

Besides the differences apparent in the above table, it will be remembered that sarcous kreatinin appears in the efflorescent form only after its solution has been kept at 60° C. for some time; whereas the natural kreatinin of urine, when prepared most carefully without heat, is always efflorescent ($C_4H_7N_3O.2H_2O$).

Also the tabular crystals formed by sarcous kreatinin are not so large as those formed by the tabular kreatinin of urine.

Having thus isolated and examined a sarcous kreatinin by the mercuric chloride method, my attention was turned in the next place to the examination of the filtered solutions from which the spherical mercury salts of the sarcous kreatinin had been separated. These filtrates were three in number, viz., from portions A, B, and C. In each case, the filtrate was allowed to stand for at least a week, in order to ensure that it remained perfectly clear. This having been ascertained, my next endeavour was to separate the mercuric chloride from the solutions, if possible, without adding any reagent to them which would be likely to alter the organic constituents during subsequent evaporation.

Finally, I effected this separation by means of pure lead hydrate, $Pb(HO)_2$. When lead hydrate is added in excess to solution of mercuric chloride, a yellow substance remains undissolved, and, on filtering after a short time, the filtrate is found to be pure water, all traces of lead, mercury, and chlorine remaining in the undissolved matter. Here, then, was a method which removed the $HgCl_2$ with-

out adding anything to the solution. Pure lead hydrate was accordingly added to all three filtrates.

In portion A more time was required to remove all mercuric chloride from the solution than in the case of portions B and C.

Lead hydrate was first added to portion A in April, 1890; and, although more lead hydrate was stirred in from time to time, the solution was not free from mercury until March 23rd, 1891. It was then filtered and evaporated.

The filtrate (portion A was that which was extracted within seven hours of the death of the animal) was evaporated first over steam, then on a hot copper plate at 60° C. No brown colour was developed in the solution during the concentration by heat, but it remained colourless, even when reduced to a syrup. This concentrated liquor was left standing over sulphuric acid.

No kreatine crystals were formed in portion A, but a number of octahedral crystals separated out. These crystals contain potassium, chlorine, and much nitrogenous organic matter. Their solution does not respond to Engel's test for kreatine with mercuric chloride and potassium hydrate.

The aqueous solution of these crystals is neutral to litmus. When heated, the crystals lost 14·7 per cent. of their weight, leaving a black ash consisting of potassium chloride, entangling carbon. I have a large quantity of these octahedral crystals, and I hope soon to submit them to complete analysis and investigation. No PO_4 was found in this part of portion A.

In the case of portion B, the separation of the mercuric chloride from the solution by lead hydrate was completed in three weeks.

The filtrate was evaporated over steam. Only slight darkening of colour took place during concentration. The yellow syrupy liquid deposited crystals on standing. The syrup was mixed with alcohol (in which the crystals did not dissolve) and filtered. The crystalline matter was washed with dilute alcohol, dried, and weighed; its weight was 2·24 grams. Having been weighed, the crystals were redissolved and recrystallised, when they were found to be pure KH_2PO_4. No kreatine crystals could be detected. The alcoholic liquor was, of course, preserved for further examination.

In the case of portion C, six weeks' digestion with lead hydrate removed the mercuric chloride from the filtrate from the spherical mercury salt of the sarcous kreatinin. The liquid was filtered from the mixed lead and mercury precipitate, and concentrated by evaporation over steam and then at 60° C.

Unlike portions A and B, portion C became extremely dark-coloured during evaporation, and the product was a brown jelly, entangling much crystalline matter.

This residue was well stirred with dilute alcohol until only the

crystals remained undissolved. The alcoholic liquor was then filtered. The filtrate was preserved, and the crystals, having been drained and washed with alcohol, were recrystallised from watery solution. The recrystallised product was evidently kreatine, and its weight was 16·63 grams. This kreatine was once more recrystallised from water, and 0·2430 gram of the product (air-dried) was kept at 100° C. till its weight was constant. The previously transparent crystals became opaque during this treatment, and lost 0·0290 gram of H_2O, or 11·93 per cent. of their weight. Kreatine, $C_4H_9N_3O_2.H_2O$, loses 12·08 per cent. at 100° C.

Therefore, the portion of the extract of meat prepared from flesh which had been most exposed to bacterial action became brown during evaporation and deposited crystals of kreatine in abundance, whilst the portions first extracted gave no kreatine, and did not become brown during concentration.

It will be well to summarise these results in tabular form.

Portion A.

Weight of flesh taken..................	30 lb. (13·62 kilograms).
Exposure to air before $HgCl_2$ added....	7 hours.
Volume of cold saturated $HgCl_2$ added..	3750 c.c.
Weight of spherical Hg salt of kreatinin	44·16 grams.
Time required to remove $HgCl_2$ by $Pb(HO)_2$	11 months.
Darkening of colour during evaporation by heat of filtrate after $Pb(HO)_2$....	None.
Kreatine obtained	None.
Crystalline product..................	An octahedral compound.

Portion B.

Weight of flesh (second extract from same portion of flesh as A)	30 lb. (13·62 kilograms).
Exposure to air before $HgCl_2$ added....	26 hours.
Volume of $HgCl_2$ solution added	2250 c.c.
Weight of spherical Hg salt of kreatinin	31·68 grams.
Time required to remove $HgCl_2$ by $Pb(HO)_2$	3 weeks.
Darkening of colour during concentration	Very slight.
Kreatine obtained	None.
Crystalline product	KH_2PO_4.

Portion C.

Weight of flesh	86 lb. 4 ozs.
Exposure to air before $HgCl_2$	34 hours.
Volume of $HgCl_2$ solution.............	8000 c.c.
Weight of spherical Hg salt..........	76·5 grams.
Time required to remove $HgCl_2$	6 weeks.
Darkening of colour during evaporation	Very great.
Kreatine obtained	16·63 grams.

The deductions which I am inclined to draw from these results are:—

1. That kreatine is not present in fresh muscle substance, but that it is a product of bacterial action upon some constituent of the flesh.
2. That the source of kreatine obtained from flesh is either the sarcous kreatinin or some closely-allied substance.
3. That sarcous kreatinin is *probably* a true "*educt*," *i.e.*, is really present in the fresh muscle-substance; but, having regard to the extremely slow separation of its mercury salt, it is just possible that it may result from gradual changes effected in some closely allied substance by the prolonged action of solution of mercuric chloride.

In conclusion, I will briefly record some experiments which I have made to ascertain whether kreatine may be converted into other bases by the prolonged action of mercuric chloride in aqueous solution at the ordinary temperature. It is commonly asserted that aqueous solution of kreatine is not acted upon by mercuric chloride. This statement, however, requires modification. It is true that there is no instantaneous action, but, after standing for 24 hours, a slight cloud forms in a mixed aqueous solution of kreatine and mercuric chloride. This precipitate increases week after week, and month after month, and is the mercury salt of kreatinin (spherical).

0·5 gram of pure kreatine, dissolved in 70 c.c. of water and mixed with 20 c.c. of cold saturated solution of mercuric chloride, deposited in the course of five months 0·7591 gram of spherical mercury salt of kreatinin, from which a tabular kreatinin was obtained in well-formed crystals. This tabular kreatinin formed a beautiful gold salt, which left, on ignition, 43·63 per cent. of gold.

Required for $C_4H_7N_3O.HCl.AuCl_3$, 43·46 per cent. Au.

This gold salt resembled that of the tabular kreatinin obtained from urinary kreatine by Liebig's process, in being *decomposed by ether*, $AuCl_3$ being dissolved and the kreatinin hydrochloride left.

It is certain that this conversion of kreatine into kreatinin does not take place when mercuric chloride is added to the watery extract of flesh—

1. Because abundance of kreatine is obtained after this treatment, as in portion C.
2. Because the kreatinin obtained from flesh differs in properties from the one obtained by action of $HgCl_2$ upon pure kreatine, the gold salt of the former kreatinin being soluble in ether, and decomposed only during evaporation; whilst the gold salt of the latter is insoluble in ether, but is at once decomposed thereby.

"Contributions to the Chemistry of Chlorophyll. No. IV." By EDWARD SCHUNCK, F.R.S. Received June 16,—Read June 18, 1891.

Action of Alkalis on Phyllocyanin (*continuation*).—In Parts I and III of this memoir I gave an account of the action of aqueous alkalis on phyllocyanin, and of the products thereby formed.* By the action of caustic alkali in a state of fusion phyllocyanin undergoes a more profound decomposition, leading to the formation of several products, one of which I shall now describe.

When caustic potash lye to which phyllocyanin has been added is boiled down nearly to dryness a green mass is left which still contains phyllocyanin, for on dissolving a little of it in water, adding an excess of acetic acid, and shaking up with ether, a solution is obtained which shows the spectrum of phyllocyanin. On heating the green mass to near the point of fusion its colour suddenly changes to brown, and the phyllocyanin is now completely altered. In order to ensure complete decomposition water is added, and the solution is then boiled down, and the residual mass again heated to near the point of fusion. The mass is then dissolved in water, and to the reddish-brown solution there is added an excess of acetic acid, which gives a voluminous brown precipitate. The whole is now shaken up with ether without any previous filtration. The ether dissolves a portion of the precipitate, acquiring a red colour, and, having been separated in the usual manner, is slowly evaporated. During evaporation the solution deposits a dark-brown mass, which is filtered off and treated with boiling alcohol. The latter acquires a red colour, leaving behind a good deal of impurity, which is filtered off. On adding zinc acetate to the filtrate a brown precipitate falls, while the liquid acquires a bright purple colour. The latter after filtration is

* 'Roy. Soc. Proc.,' vol. 39, p. 355, and vol. 44, pp. 448—454.

evaporated. During evaporation it deposits an amorphous red powder, which is filtered off and washed with a little cold alcohol. This red powder is a zinc compound; when burnt it leaves a residue of zinc oxide. On being treated with hot alcohol to which a little hydrochloric acid is added the powder dissolves, yielding a crimson solution, which after adding water and shaking up with ether divides into two layers, an upper brown ethereal one and a lower one which is acid and has a bright crimson colour. The two liquids having been separated, the upper one is found to leave on evaporation a brown amorphous residue, while the lower one, after partial evaporation, gives with water a brown precipitate. The latter, having been filtered off, is dissolved in boiling alcohol; the alcoholic solution deposits on standing fine lustrous crystalline needles.

The properties of the substance prepared as just described are as follows:—In mass it appears plum-coloured, and shows much lustre. Under the microscope it is seen to consist of small, regular, prismatic crystals, which are reddish-brown by transmitted light. Heated on platinum it melts to a brown mass, which on being further heated burns, leaving much charcoal. When heated in a tube it gives off fumes, but yields no crystalline sublimate. It is soluble in boiling alcohol and ether, the solution having a red colour inclining to crimson. It is also soluble in chloroform, but insoluble in carbon disulphide even on boiling. It dissolves in glacial acetic acid and in concentrated hydrochloric acid, the solutions exhibiting a fine crimson colour. The solutions show well-defined absorption bands, but their spectra differ *inter se;* those of the alcoholic and ethereal solutions are nearly alike, but that of the solution in hydrochloric acid is very different. The spectra are remarkable for the entire absence of bands at the red end, but they need not be further described, as they are represented on the plate accompanying this paper.

I give no name to this substance, as it may possibly turn out to be identical with one or other of the bodies obtained by Hoppe-Seyler as products of the action of caustic potash on his chlorophyllan,* one of which he has named "dichromatic acid," while to another he applies the term "phylloporphyrin." I may state that the spectrum of the hydrochloric acid solution of my substance corresponds exactly with that of a purple-coloured solution (containing unfortunately very little substance) labelled "phylloporphyrin," which I owe to the kindness of Dr. Schuchardt, of Goerlitz. The yield of my substance in relation to the quantity of phyllocyanin employed is exceedingly small, as the account of my method of preparing and purifying it would, indeed, lead one to conclude. Large quantities of one or more other substances are formed at the same time, but, being brown, amorphous, and humus-like, they do not invite examination.

* 'Zeits. f. Physiol. Chem.,' vol. 4, Heft 3.

This may, I think, be a suitable place for a few remarks on the somewhat altered sense in which I use the terms phyllocyanin and phylloxanthin, names bestowed by Fremy on what he supposed to be constituents of chlorophyll. In his first memoir on the green colouring matter of leaves Fremy* maintained that by acting on chlorophyll with a mixture of ether and hydrochloric acid he had caused it to split up into two colouring matters, one of which passes into the acid, imparting to it a bright blue colour, while the other dissolves in the ether, which it colours yellow, the two together causing by their combined presence the ordinary green colour of leaves and other organs. I need not here refer to the fact, sufficiently manifest to any one who has paid attention to the subject, that Fremy was in error in supposing that his two colouring matters pre-existed as such in the cells of plants, and was not aware that they were, in part at least, products of the decomposition of chlorophyll. I merely wish to remark that the author's two colouring matters must have been, considering the mode of preparation employed, mixtures of several substances, some of which, so far as we know, are not in any way connected with chlorophyll, using the latter term in the stricter sense as being the substance to which the green colour of leaves, &c., is due. At the same time I think it right to say a few words to justify myself in retaining Fremy's names, and at the same time applying them in a somewhat different sense. Returning to Fremy's first experiment with ether and hydrochloric acid, I think I have proved, by what is stated in the first part of this memoir, that the author's blue liquid is in fact a solution in hydrochloric acid of a substance which is a product of decomposition of chlorophyll. This substance I call phyllocyanin. It is a weak base, forming with acids unstable compounds of a blue colour, Fremy's phyllocyanin being in fact such a compound. It also combines with bases, though not without at the same time undergoing a change, a description of which is contained in the third part of this memoir.† To have given it another name would probably have led to misunderstanding on the part of those not specially conversant with the subject, and to confusion in its terminology. A similar instance of the retention of name presents itself in the case of the alkaloid berberin. The berberin of the original discoverer was subsequently found to be the hydrochloride of a base which then retained the name berberin, though not identical with the original substance so called. In his second memoir‡ Fremy takes quite another view of the constitution of chlorophyll. Having acted on chlorophyll in alcoholic solution with caustic baryta he obtained an insoluble compound, which he called phyllocyanate of baryta, and

* 'Comptes Rendus,' vol. 50, p. 405.

† 'Roy. Soc. Proc.,' vol. 44, p. 448.

‡ 'Comptes Rendus,' vol. 61, p. 188.

which by decomposition with acid yielded phyllocyanic acid, while his phylloxanthin remained dissolved in the alcoholic liquid, and was left behind on evaporation in yellow or red crystals resembling bichromate of potash. This process the author considers to be one of saponification, chlorophyll itself being a species of fat, which by the action of bases splits up, the process being described by him as follows:—"La chlorophylle, espèce particulière de corps gras coloré, éprouve donc par l'action des bases énergiques une sorte de saponification dont la phylloxanthine, corps neutre jaune, serait la glycérine, et l'acide phyllocyanique serait l'acide gras coloré en vert bleuâtre." The process may, however, be explained in a simpler manner. On coming into contact with caustic baryta chlorophyll simply combines with the latter forming an insoluble compound, and is again set at liberty by a strong acid, but is at the same time decomposed by the acid yielding phyllocyanin, the so-called phyllocyanic acid being simply identical with the latter. The properties of his acid, as described by the author, are just those of phyllocyanin in my sense of the term. It is hardly necessary to refer here to Fremy's third memoir,* in which he endeavours to prove that the colouring matter of leaves is a mixture of phylloxanthin and phyllocyanate potash, of a conclusion even less probable than those previously arrived at.

As regards Fremy's phylloxanthin, it is evident that it must, considering the method of preparation employed, have consisted of several more or less yellow substances, some of which may have been present as such in the solution of chlorophyll taken, while others were formed by the action of the hydrochloric acid on the chlorophyll in solution. In his first memoir Fremy states that his phylloxanthin is, in his opinion, identical with the yellow colouring matter of leaves developed in the dark as well as that of yellow autumnal leaves. Whether the colour of etiolated and that of faded leaves is due to the same substance, and whether the latter is identical with the yellow colouring matter which always accompanies chlorophyll in healthy green leaves is uncertain, but there can be no doubt that the latter, the constant companion of chlorophyll, was present in Fremy's phylloxanthin solution. Though still an imperfectly known substance, its general properties have been ascertained, and it is now usually called xanthophyll. In his second memoir Fremy gives a description of his phylloxanthin, from which it is evident that the substance obtained by his new process was identical with Hartsen's chrysophyll and Bougarel's erythrophyll, a body yielding beautiful red crystals, giving solutions of a deep yellow colour, and apparently present in all green leaves. This then is the second constituent of Fremy's phylloxanthin, but there is yet a third, which, though overlooked by that chemist, must have been present along with the two others, at

* 'Comptes Rendus,' vol. 84, p. 983.

least when the process first described, using ether and hydrochloric acid, was employed; and it is this, following the example of Tschirch, that I propose to call phylloxanthin in the stricter sense, inappropriate as the term is in some respects.

Phylloxanthin is a product formed along with phyllocyanin by the action of strong acids on chlorophyll, and is left dissolved in ether when to an ethereal solution of the two concentrated hydrochloric acid is added, the phyllocyanin passing into the acid. When the process described in the first part of this memoir, the first stage of which consists in passing hydrochloric acid gas into an alcoholic extract of leaves, is employed, the xanthophyll, chrysophyll, and any other colouring matter that may be present remain in solution, while the phyllocyanin and phylloxanthin formed by the action of the acid are deposited along with some fatty matter.

Having thus explained the sense in which the terms phyllocyanin and phylloxanthin are employed in this memoir, I will now proceed to give an account of the mode of preparation and properties of

Phylloxanthin.

Although the quantity of this substance formed by the decomposition of chlorophyll with acids is much larger than that of the phyllocyanin accompanying it, its preparation in a state of purity is much more difficult, and the product even at the best is never free from impurities of a fatty nature.

The olive-green ethereal solution containing phylloxanthin, after being separated from the lower acid one containing phyllocyanin, and shaken up with concentrated hydrochloric acid, so as to remove any of the latter substance that may be present, is left exposed to the air in shallow vessels, so as to allow the greater part of the ether to evaporate, when it leaves a quantity of matter in dark brown cakes floating in an acid liquid. The latter having been removed, the cakes are washed with water, and then left to drain on paper. That the mass thus obtained contains, besides colouring matter, a large quantity of fat is evident, for on rubbing a small portion between the fingers it softens, and it melts completely in boiling water. The fatty matter may, to a great extent, be removed by treating the mixture with concentrated hydrochloric acid, in which the colouring matter after a time dissolves leaving the fatty matter behind; but the former undergoes a change by this treatment, and is no longer unaltered phylloxanthin, as I shall show further on. By the action of alkalis, too, a change is effected, so that it is necessary to use other solvents for the purification of phylloxanthin. For this purpose the crude substance from the ethereal solution is first dissolved in a small quantity of chloroform, and the solution is then mixed with several

times its volume of alcohol and left to stand, when it deposits a great part of the phylloxanthin, much of the fatty matter being left in solution.

The deposit is then filtered off, washed with alcohol, dried, and dissolved in boiling glacial acetic acid. This solution on cooling and standing yields a copious deposit of phylloxanthin, which is filtered off and may again be dissolved in boiling glacial acetic acid. The second deposit is filtered off, washed with alcohol, and dissolved in hot ether. The ethereal solution is then slowly evaporated in beakers. During evaporation a dark mass is deposited, which after about three-fourths of the ether have evaporated is filtered off and redissolved in ether. The ethereal solution is evaporated, and the portion which first separates out is filtered off as before. This process may be repeated several times if it is thought necessary. The quantity finally obtained after this lengthy process is, of course, relatively small. Unfortunately, too, it is after all not absolutely pure; it contains fatty matter, as may be easily ascertained by treating a little of it with moderately strong boiling nitric acid, when the colouring matter is decomposed and dissolved, leaving the fatty matter, which floats about in the boiling liquid in oily drops. There is no reason, however, to suppose that this contamination with fatty matter interferes with the reactions of the substance, so far at least as its behaviour to various solvents and the colour or changes of colour which it presents are concerned. The properties of phylloxanthin resemble those of phyllocyanin so closely as to lead to the conclusion that the two substances must be closely related, that they are perhaps isomeric, and that possibly one of them might be converted into the other by some process still to be discovered. In the following description, therefore, I shall refer only slightly to those properties which the two substances possess in common.

When prepared in the way above described and then dried phylloxanthin appears dark green, almost black, thus differing from phyllocyanin, which always shows a dark indigo-blue colour. It is amorphous, even under the microscope, though it may occasionally be obtained by very slow evaporation of its ethereal solution in small rosettes, which are rust-coloured by transmitted light. When a very minute portion is placed on a glass slide, then moistened with ether under a cover-glass, it is seen to resolve itself under the microscope into a number of long whip-like filaments and pseudo-crystalline needles, much curved and twisted, which are brown by transmitted light. Chlorophyllan, according to Hoppe-Seyler, shows the same behaviour under the microscope.

Phylloxanthin is soluble in boiling absolute alcohol, but a great part separates out again on the solution cooling as a granular amorphous deposit. It is more soluble in ether, carbon disulphide,

benzol, and aniline than in alcohol; it also dissolves in ligroin, but the best solvent is chloroform. These solutions are less distinctly green, and more brown, than those of phyllocyanin; they are fluorescent, and when dilute exhibit a marked reddish tinge, of which nothing is seen in the case of phyllocyanin. The ethereal solution shows five bands closely resembling those of phyllocyanin, as regards both position and relative intensity, with this difference, however, that the first and second bands lie further away from the red end than with the latter, while the space between the fourth and fifth bands is so much darkened that when the solution is concentrated the two bands appear as one. When the ethereal solution is shaken up with concentrated hydrochloric acid, the latter remains colourless, unless phyllocyanin is present, when it acquires a blue colour. It would, however, be a mistake to suppose that if, on standing, a blue coloration makes its appearance in the lower liquid, the presence of phyllocyanin is indicated, for by the action of hydrochloric acid phylloxanthin undergoes a change whereby it becomes soluble in the acid, leaving the ether in which it was previously dissolved. That this is the case is evident from the fact that the blue coloration commences where the ether and the acid are in contact, and extends from above downwards.

Phylloxanthin, like phyllocyanin, may be heated for several hours at 130° without undergoing any change, but at 160° decomposition commences, and at 180° the substance is completely decomposed, yielding a charred mass, which is entirely insoluble in chloroform. When heated on platinum phylloxanthin burns with a luminous flame yielding a bulky charcoal, which burns away with difficulty, leaving a little dark-coloured ash; this ash consists of ferric oxide.

Since all the specimens of phylloxanthin I have prepared leave on burning more or less ferric oxide, the presence of the latter cannot be altogether accidental. Those who maintain that iron in some form or other is an essential constituent of chlorophyll may not be surprised at its being found in one of the products of the decomposition of chlorophyll; but it is difficult to understand why the iron, if originally present, should not have been removed during the treatment with hydrochloric acid and acetic acid to which the product was submitted, and should still be present after repeated solution of the substance in ether.

On adding a little nitric acid to a saturated solution of phylloxanthin in glacial acetic acid the colour of the solution changes to a deep yellow; the solution deposits nothing on cooling; it gives with water a brown flocculent precipitate, the filtrate from which is yellow, and shows no absorption bands, while the precipitate after washing dissolves in alcohol, giving a yellow solution which shows absorption bands, and leaves on evaporation a yellow brittle amorph-

ous residue. Chromic acid acts in a similar manner. Treated with boiling concentrated nitric acid, phylloxanthin is decomposed, a quantity of yellow semi-fused fatty matter being left behind; the filtrate leaves on evaporation a crystalline residue, consisting mainly of oxalic acid.

A chloroformic solution of phylloxanthin when exposed to sunlight in a loosely stoppered bottle gradually becomes paler, and at last almost colourless. No difference in the rapidity of bleaching could be observed when a solution of phyllocyanin of as nearly as possible the same strength was exposed to the action of light along with the solution of phylloxanthin. The process is doubtless one of slow oxidation under the influence of light.

On adding a little bromine to a chloroformic solution of phylloxanthin the colour of the latter changes to a bright grass-green, resembling that of chlorophyll solutions, but its spectrum differs from that of chlorophyll, as well as from that of phylloxanthin; it shows one very broad band, extending over a great part of the red and the whole of the orange, with a faint band in the green, and an indication of one nearer the blue, together with much obscuration at the blue end. The solution leaves on evaporation a little amorphous residue, which is purple by reflected, and dark green by transmitted, light, and contains bromine. On adding an excess of bromine to a chloroformic solution of phylloxanthin the colour of the solution changes to a deep yellow, but it still shows a band in the red.

On treating phylloxanthin with concentrated hydrochloric acid in the cold no change takes place at first, but after some time a great part of the substance dissolves, yielding a dark greenish-blue solution, while part remains undissolved as a green fatty mass, which may be separated by filtration through asbestos. The filtrate appears slightly more green than a solution of phyllocyanin in hydrochloric acid, but it shows nearly the same spectrum as the latter, though the bands of which it consists are far less distinct and well marked. The solution, when shaken up with ether, imparts no colour to the latter, behaving in this respect like a solution of phyllocyanin, which shows that phylloxanthin by treatment with hydrochloric acid undergoes some change. Nevertheless, when the product of the action is precipitated from the acid solution by water the precipitate, after filtering off and washing, dissolves in ether, yielding a solution which shows the colour and absorption spectrum of a solution of phylloxanthin. The precipitated product dissolves also in glacial acetic acid boiling, and the solution on cooling deposits a portion which, when filtered off and dried, appears dark blue, like phyllocyanin, and when viewed under the microscope shows the same indistinctly crystalline form as the latter. In all other respects, however, it gives the same reactions as phylloxanthin, so that it is certain no trans-

formation into phyllocyanin is effected by the action of acid. The behaviour of phylloxanthin to concentrated sulphuric acid closely resembles that just described.

When cupric acetate is added to a solution of phylloxanthin in boiling glacial acetic acid the solution becomes dark green, and a compound is formed very similar to that which phyllocyanin yields when treated in the same way. The solution, on cooling and standing, gives a dark-coloured deposit, which, after filtering off and treatment with dilute hydrochloric acid, so as to remove any excess of cupric acetate that may be present, is redissolved in boiling glacial acetic acid. From this solution the compound crystallises out in small scales, which are purple and lustrous by reflected light and pale green by transmitted light. It closely resembles the corresponding phyllocyanin compound; its solutions show the same absorption spectrum as those of the latter, though they have less of a blue tint, and appear more green. When the same experiment is made using zinc acetate instead of cupric acetate no change of colour takes place, and the acetic acid solution on cooling deposits unaltered phylloxanthin; in this respect the two substances show a marked difference, since phyllocyanin yields a double compound with zinc and acetic acid, as described in Part I of this memoir.

When ferrous oxide and argentic oxide are employed along with phylloxanthin and acetic acid compounds are formed similar to those yielded by phyllocyanin, but their properties are not of sufficient interest to merit detailed description. Towards lead acetate phylloxanthin, like phyllocyanin, behaves with complete indifference.

When metallic tin is added to a solution of phylloxanthin in concentrated hydrochloric acid the phenomena are similar to those observed in the case of phyllocyanin. After standing some time the bright bluish-green colour of the solution changes to olive, and it now gives with water a brown precipitate. On allowing the hydrochloric acid solution to stand in contact with metallic tin for some time longer, it becomes red, and now gives with water a red flocculent precipitate, which on being filtered off and washed turns brown, and then dissolves in alcohol with a brown colour; the alcoholic solution shows only a faint band in the green, but on the addition of a little caustic alkali it turns yellow, and now shows three bands, which are not, however, very distinct.

Phylloxanthin does not dissolve readily in aqueous alkali, but it does so with ease when alcoholic potash or soda is employed. When treated with boiling alcohol to which a little alcoholic potash is added phylloxanthin dissolves at once and entirely, yielding a red solution, which on continuing to boil turns green. On standing for some time the solution gives a dark-coloured deposit, which is filtered off, washed with alcohol, and dissolved in water. The watery

solution gives with acid a greenish-brown precipitate, which is filtered off and dissolved in a little boiling glacial acetic acid. The solution on being left to stand for some days yields a deposit, which is filtered off, washed with a little alcohol, and dried. This product corresponds to the phyllotaonin obtained by the action of alkalis on phyllocyanin, but its properties, though similar, are less characteristic. When dry it appears almost black and amorphous; under the microscope, however, it is seen to consist in part of prismatic crystals. It is soluble in alcohol, ether, chloroform, and glacial acetic acid, but is insoluble in petroleum ether. The solution in ether has a pink colour, and shows a spectrum consisting of four bands, of which two, viz., one in the red and the other in the green, are very dark. On diluting the solution until only a slight tinge of colour is left, the band in the green becomes very faint, while that in the red splits up into two nearly equal narrow bands; the band in the green remains single, however much the solution may be diluted, whereas the analogous derivative of phyllocyanin, after its solution has stood for some time, shows a characteristic double band in this part of the spectrum.

The description of phylloxanthin just given shows that the properties of the substance closely resemble those of its companion phyllocyanin. Until the composition of the two substances has been ascertained, it is impossible to say in what relation they stand to one another; it is probably a case of isomerism. All attempts to transform phylloxanthin into phyllocyanin, or *vice versâ*, failed. It is certain, however, that the two substances are not formed simultaneously, unless strong acids, such as hydrochloric, are employed. When, for instance, a little acetic acid is added to an ethereal solution of chlorophyll there is an immediate change of colour in the solution accompanied by the formation of phylloxanthin; it is only after some time that phyllocyanin makes its appearance; at least, such is the conclusion derived from spectroscopic examination of the solution.

This phenomenon may be explained by supposing that the two substances phyllocyanin and phylloxanthin are formed by the decomposition of two distinct bodies. The researches of Stokes, Sorby, and others have led to the conclusion that ordinary chlorophyll is a mixture of several colouring matters, two of which Mr. Sorby has named "blue chlorophyll" and "yellow chlorophyll" respectively. In a written communication which Sir G. Stokes has kindly addressed to me, he informs me that he has satisfied himself that by decomposition with acids blue chlorophyll yields phyllocyanin, whereas yellow chlorophyll gives phylloxanthin. This interesting fact affords a striking confirmation of the views held by him regarding the complex nature of chlorophyll.

The literature of chlorophyll contains descriptions of several sub-

stances the properties of which closely resemble those of phylloxanthin. One of these is Pringsheim's "hypochlorin." After immersing tissues containing chlorophyll in dilute hydrochloric acid, Pringsheim observed the formation, after some time, of peculiar brown crystalloid, sometimes even crystallised, bodies, attached to, and proceeding from, the chlorophyll corpuscles of the cells, and which he supposed to pre-exist in the latter, the acid merely serving to eliminate them. They constitute his hypochlorin. I have repeated Pringsheim's experiments with the leaves of various plants, and found the phenomena under the microscope exactly such as he describes. On examining the properties of the hypochlorin obtained, more especially the absorption spectrum of its solution, I arrived at the conclusion that they do not differ from those of phylloxanthin, and that the two substances are, in fact, identical.

Action of Alkalis on Chlorophyll.

The action of alkalis on chlorophyll has been less frequently and less minutely studied than that of acids. This may be easily accounted for, seeing that, by the prolonged action of alkalis on solutions of chlorophyll, no marked changes as regards colour or other outward properties take place in the latter, some authors going so far as to say that no change whatever is effected by the action, the chlorophyll merely combining with the alkali to form a saline compound. That alkalis do not effect so profound an alteration in chlorophyll as acids do is true; still, there can be no doubt that the former do lead to the formation of a substance having quite distinct properties, as I think I shall succeed in showing.

Chautard,* who was, I believe, the first to study the action of alkalis on chlorophyll, states that when caustic potash is added to an alcoholic solution of chlorophyll, which is then heated, a change takes place in the spectrum, which consists in the splitting into two of the band in the red, called by him "bande spécifique," the addition of an excess of acid causing it to appear single again, the doubling reappearing with alkalis, and so on. The fainter bands of the chlorophyll spectrum disappeared almost entirely in Chautard's experiment.

On adding potash or soda to a chlorophyll solution Russell and Lapraik† observed a change in the spectrum, which consisted in the fading out of all except the least refrangible or dominant band, the latter at the same time spreading towards the blue. With a considerable excess of alkali the dominant band divided into two distinct bands, in accordance with the observation of Chautard.

Tschirch‡ devotes a whole chapter of his "Investigations" to an

* 'Comptes Rendus,' vol. 76, pp. 570, 1273.

† 'Journal of the Chemical Society,' vol. 41, p. 334.

‡ 'Untersuchungen üb. das Chlorophyll,' Berlin, 1884.

account of the action of alkalis on chlorophyll. The conclusion he arrives at is that the action induces a complete change in the chlorophyll, leading to the formation of a distinct substance which he calls "chlorophyllic acid." The compounds of this substance with alkalis show, when in solution, two bands in the red, one of them being very thin. By the action of hydrochloric acid chlorophyllic acid undergoes decomposition; the ethereal solution of the product of decomposition shows a spectrum which is, apparently, that of phyllocyanin.

Hansen in his memoir, entitled "Der Chlorophyllfarbstoff,"* assumes that chlorophyll undergoes no change by the action of alkalis, and even submits the crude colouring matter to a process of saponification in order to obtain it in a state of purity. The final product obtained by Hansen appeared in sphæro-crystals, and consisted, according to him, of chlorophyll, or, as he prefers to call it, "chlorophyll-green," in the highest possible state of purity. The fact that its solutions show the ordinary chlorophyll spectrum would seem to prove the accuracy of this view. Nevertheless, it is evident from the description of its mode of preparation and properties that chlorophyll-green must have been a sodium compound; and this, indeed, is acknowledged by the author himself who, in a subsequent memoir,† describes a process for obtaining the colouring matter free from sodium, which, from the fact of the substance being very sensitive to the influence of acids, must be conducted with care.

In a memoir, entitled "Extraction de la Matière Verte des Feuilles,"‡ Guignet describes a method for obtaining the sodium compound of chlorophyll in dark green, crystalline needles, of which he says that its solutions show exactly the same absorption bands as those of ordinary chlorophyll. I have repeated the experiments of Hansen and Guignet, but have not succeeded in obtaining a crystalline compound as they did, probably from want of manipulative skill on my part. The product prepared by Guignet's process, though amorphous, did, however, on decomposition with acid yield a result which plainly showed that the body acted on was by no means unchanged chlorophyll; the product of decomposition gave the reactions not of phyllocyanin or phylloxanthin, but of phyllotaonin.

In order to confirm or otherwise the results arrived at by my predecessors, I devised a new and comparatively simple method of preparing what has been called "alkaline chlorophyll." The process I adopt may be described as follows:—Fresh leaves, preferably of grass, are treated with boiling spirits of wine containing about 80 per cent. of alcohol. The green extract is filtered hot and, after standing for a day or two, yields a dark green voluminous deposit containing

* 'Arbeiten d. Bot. Instituts in Würzburg,' vol. 3, p. 1.

† 'Arbeiten d. Bot. Instituts in Würzburg,' vol. 3, p. 3.

‡ 'Comptes Rendus,' vol. 100, p. 434.

a considerable portion of the chlorophyll of the extract along with other matters. The deposit is filtered off and then treated with a boiling solution of caustic soda in strong alcohol. The boiling should be continued for some time before the liquid is filtered. Through the dark green filtrate a current of carbon dioxide is then passed for some time. The gas produces a green deposit which settles rapidly, the liquid only retaining a pale yellowish-green colour. After standing some time the deposit is filtered off; it consists in great part of crystalline particles of sodium bicarbonate, together with the product of the action of alkali on chlorophyll combined with soda. The mass on the filter is now washed with cold alcohol as long as the latter takes up colour, and is then treated with water, in which the greater part dissolves, a little nicely crystallised chrysophyll being generally left undissolved. The filtrate is now mixed with several times its volume of a saturated solution of common salt. This gives a green flocculent precipitate which settles slowly, leaving a supernatant pale green liquid. The precipitate is filtered off and washed with saturated salt solution until the percolating liquid is no longer alkaline. It is then treated with boiling alcohol, which dissolves the greater part, leaving undissolved a small quantity of green powder consisting of calcium and magnesium compounds formed by double decomposition from the calcium and magnesium chlorides with which common salt is usually contaminated. The filtered alcoholic solution leaves on evaporation a dark green residue along with a quantity of salt. The latter may be in part removed by treatment with a small quantity of cold water, and the rest remains behind when cold absolute alcohol, in which the green compound is soluble, is added. The solution in absolute alcohol is of a pure dark green, and leaves on evaporation an amorphous resin-like residue, which is purple and lustrous by reflected and bright green by transmitted light; it may be easily pulverised, yielding a dark green powder.

The product thus obtained is a sodium compound, and is soluble in water. The aqueous solution gives green precipitates with barium chloride, lead acetate, cupric acetate, and silver nitrate; the silver precipitate blackens at once on boiling the liquid. In order to obtain the substance with which the sodium is combined, the product is dissolved in water and just sufficient acetic acid is added to set the green substance combined with the sodium at liberty. The whole is then shaken up with ether, which dissolves the green flocculent precipitate, giving a dark green solution. The latter is washed several times with water in order to remove the free acid and sodium acetate, and is then evaporated spontaneously, when it leaves a residue which cannot be distinguished by its appearance from the sodium compound from which it is derived.

The substance thus obtained possesses properties by which it may

be easily distinguished from chlorophyll, and which give it the character of a substance *sui generis*. I would propose to call it, until a better name can be found, *alkachlorophyll*, a name pointing at the same time to the agent by which it is formed and to its resemblance to the mother substance. It shows no signs whatever of crystalline structure, is quite insoluble in boiling water, but easily soluble in alcohol, ether, chloroform, benzol, aniline, and carbon disulphide, but insoluble in petroleum ether. Its solutions have a brilliant green colour with a pronounced bluish tinge, by which they may be easily distinguished from ordinary chlorophyll solutions, and a marked red fluorescence. The ethereal solution shows no less than six absorption bands. Of these, the two in the red probably correspond with those first observed by Chautard, while the one in the blue is in a part of the spectrum in which total absorption takes place with ordinary chlorophyll solutions.

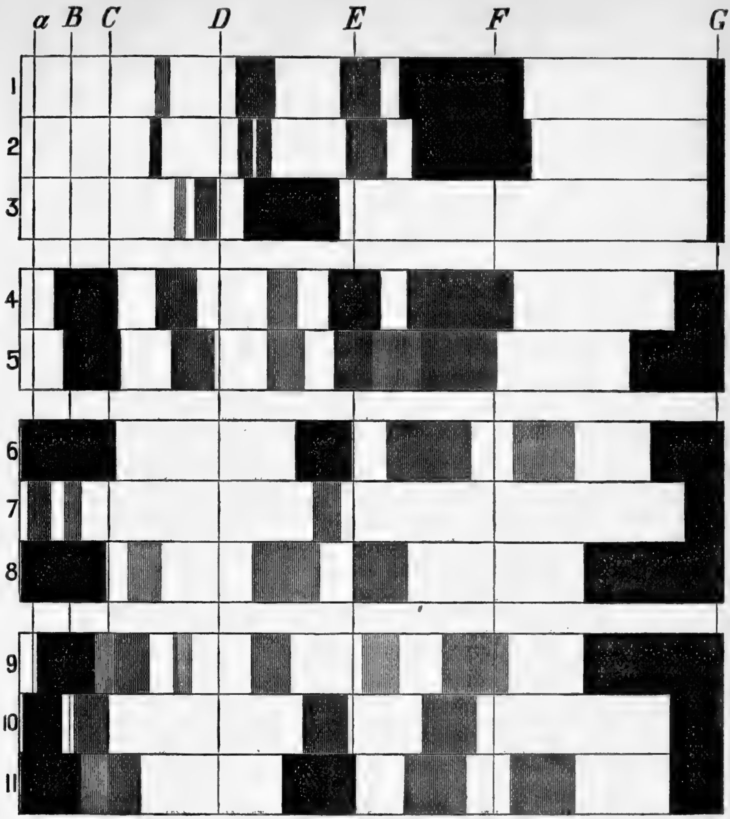
Alkachlorophyll in solution shows a remarkable degree of permanence when exposed to the combined action of air and light as compared with chlorophyll. Having prepared an ethereal solution of alkachlorophyll, and also an ethereal solution of ordinary chlorophyll, from fern fronds, as nearly as possible of the same depth of colour as the difference of tint would permit, I exposed the two solutions in loosely stoppered bottles to the action of alternate sunlight and diffused daylight in a window facing the south. The experiment commenced on the 21st February. On the 27th of the same month the chlorophyll solution had lost its green colour and become yellow, the chlorophyll bands having disappeared, those still remaining being due to phyllocyanin, the formation of which was doubtless due to decomposition of part of the chlorophyll. The solution of alkachlorophyll, on the other hand, was still green, and still showed the six original bands, though it was found to have lost in intensity of colour when compared with another portion of the same solution that had been kept in the dark. On the 11th March the solution had become pale green, and now only showed four bands. On the 25th March the solution still showed the bands in the red and blue, though they appeared very faint. On the 8th May a slight green tinge was still visible, but the bands had nearly disappeared. The relatively great stability of alkachlorophyll when exposed to air and light is of itself sufficient to prove that by the action of alkali chlorophyll undergoes a thorough change.

The action of acids on alkachlorophyll is especially interesting, because the products to which it gives rise differ entirely from those derived from the decomposition of chlorophyll with acids. When acetic acid is added to an alcoholic solution of alkachlorophyll the solution on heating loses its green colour, which changes to a dirty purple. The substance is now completely decomposed, but no phyllo-

cyanin is found among the products of decomposition, the chief product nearly resembling phyllotaonin, with which it is indeed probably identical. The solution, on being evaporated nearly to dryness, gives a dark coloured deposit, which being filtered off may be dissolved in boiling glacial acetic acid. The solution left to stand yields a semi-crystalline deposit, which, filtered off, washed with cold alcohol, and dried, appears purple by reflected light; it dissolves, though not readily, in ether, giving a purplish solution which shows the same absorption bands as phyllotaonin acetate or ethylphyllotaonin. On treatment with alcoholic potash it dissolves, giving a bright green solution which on boiling turns brown, and on now adding acetic acid and shaking up with ether a solution is obtained which has the colour and shows the same bands as a solution of phyllocyanin.

These reactions would seem to indicate the presence of phyllotaonin among the products of decomposition of alkachlorophyll; the simultaneous presence of other products, especially fatty acid, renders its purification difficult. The method of preparing alkachlorophyll as above described does not of course preclude the possibility of its containing an admixture of fatty acid; still I cannot help thinking that a portion at least of the fatty acid found is actually formed during the process of decomposition with acids.

In order to see whether, in addition to the one just described, some other product or products possibly soluble in water were formed by the action of acids on alkachlorophyll, a quantity of the substance was treated with boiling dilute sulphuric acid. The acid liquid filtered from the undissolved matter was coloured blue. On being neutralised with barium carbonate the blue colour disappeared, and the liquid having been filtered and evaporated left a slightly coloured residue which was treated with absolute alcohol. This dissolved a portion, and left after filtration and evaporation a pale yellow glutinous residue. This residue was soluble in water, but insoluble in ether. The watery solution had an alkaline reaction; it did not react with Fehling's solution. The alcoholic solution gave with platinum chloride a pale yellow crystalline deposit. The quantity of the substance obtained in this experiment was too small to allow of its identification. Should it turn out to be an organic base, as its reactions would seem to indicate, the fact would tend to confirm the view taken by Hoppe-Seyler, who obtained cholin as a product of decomposition of his chlorophyllan, and hence concluded that chlorophyll itself might have a constitution similar to that of lecithin.



Absorption Spectra of Substances described in this Paper.

1. Product of the action of melting caustic potash on phyllocyanin dissolved in alcohol.
2. The same dissolved in ether.
3. The same dissolved in concentrated hydrochloric acid.
4. Phyllocyanin in ether.*
5. Phylloxanthin in ether.
6. Product of the action of alkali on phylloxanthin in ether.
7. The same, dilute solution.
8. The same dissolved in acetic acid.
9. Alkachlorophyll in ether.
10. Product of the action of sulphuric acid on alkachlorophyll in ether.
11. Product of the action of acetic acid on alkachlorophyll in ether.

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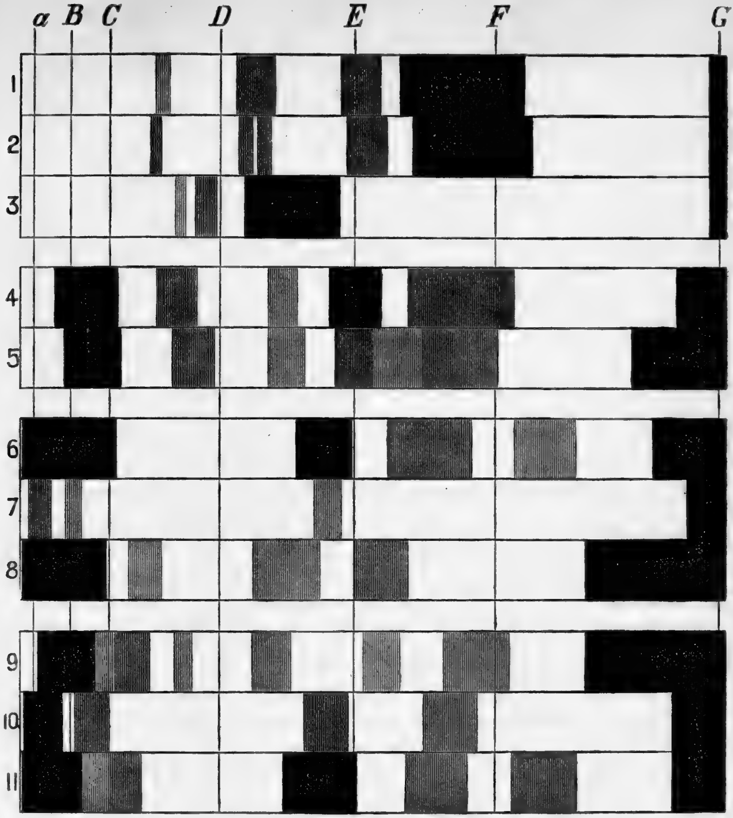
* This spectrum, which has already been figured, is here given again for the sake of comparison with the following one.



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January 14, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

The Chairman announced the lamented death of H.R.H. the Duke of Clarence and Avondale.

The Fellows determined to adjourn the Meeting forthwith, and directed the Secretaries to send, on behalf of the Society, addresses of sympathy in their deep affliction to Her Majesty the Queen and Their Royal Highnesses the Prince and Princess of Wales.

January 21, 1892.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Treasurer read a Letter which had been sent to him as Chairman of the preceding Meeting, expressing the warm thanks of the Prince of Wales for the sympathy expressed by the Fellows of the Royal Society in his affliction.

The Treasurer offered the Congratulations of the Society to the President on his elevation to the Peerage.

A List of the Presents received was laid on the table, and thanks ordered for them.

The Right Hon. Farrer Herschell, Lord Herschell, whose certificate had been suspended, as required by the Statutes, was balloted for and elected a Fellow of the Society.

The following Papers were read :—

I. "Note on the Audibility of single Sound Waves, and the Number of Vibrations necessary to produce a Tone." By E. F. HERROUN and GERALD F. YEO, F.R.S. Received November 26, 1891.

I.

When investigating the sounds produced by skeletal muscle when caused to contract at varying rates by electric stimulation, we were

thoroughly convinced that a single contraction produced by a single induction shock gave rise to an audible sound of somewhat the same character as the first sound of the heart.*

But it was urged by some physical friends that if the contraction were really a single one, causing a single vibration, it could not be heard, because a *series* of vibrations was necessary to produce a sound, and therefore, unless secondary oscillations succeeded the contraction, the latter would be inaudible.

Now muscular tissue, especially when surrounded by fascia, fat, &c., seemed to be particularly ill suited for sustaining any such series of vibrations, and no such oscillations can be detected on the graphic record of the muscular movement.

When the muscle was stimulated at regular intervals with increasing frequencies the short thuds due to each contraction could be heard separately up to a rate of about forty per second, when these thuds became gradually fused into a dull tone, only clear at somewhat above the rate now accepted as the lower limit of audibility of true tones, viz., forty-one vibrations per second. We adopt this rate, given by Helmholtz,† and neglect the rates of Savart and Preyer,‡ as we believe the tones they heard were probably due to harmonic additions to the rumble of single sound waves caused by 8 or 15 V.D. per second. That the droning sound produced by the lowest organ pipes is not really heard as a true tone seems satisfactorily proved by the well-known fact that they cannot be tuned by the ear alone even of the greatest expert, but only indirectly by the beats which they make with the tones of the upper octaves. The value of such sounds as those produced by a body vibrating slower than forty times per second, and having no true tone capable of differentiation of pitch, can only be to modify and soften the tones of higher octaves, which so occupy the auditory apparatus as to make the separation of the slow single vibrations no longer perceptible.

But the question of the vibrations becoming fused into a musical tone is distinct from that of the audibility of each vibration separately, except in so far as the admission of imperfect union distinctly implies the audibility of the separate vibrations.

To us there did not seem to be the least physiological difficulty in

* 'Journal of Physiology,' vol. 6, p. 287.

† 'Die Lehre v. d. Tonempfindungen,' &c., Braunschweig, 1870, p. 278: "In der künstlerischt vollendeten Musik des Orchesters ist deshalb auch der tiefste Ton welcher angewendet wird, das E, des Contrabasses von 41 Schwingungen, und ich glaube mit Sicherheit voraussagen zu können, das alle Anstrengunen der neueren Technik, tiefere gut musikalische Töne hervorzubringen, scheitern müssen, nicht weil es an Mitteln fehlte passende Luftbewegungen zu erregen, sondern weil das menschliche Ohr seine Dienste versagt."

‡ 'Physiologische Abhandlungen,' Theil 1, pp. 1—17.

the belief that a single wave could be transmitted, so as to excite the terminals of the auditory nerve.

On the other hand, if one complete vibration does not stimulate the hearing apparatus, a distinct logical difficulty presents itself when we attempt to explain how multiples of no stimulation can give rise to such an immense variety of definite effects on the cochlea and the brain. If expressed as a formula, the inaudibility of single vibrations appears absurd, 1 V.D. = 0 but 528 V.D. = C″.

Being satisfied that a single contraction of muscle could be heard as a thud, and that the single thuds could be heard separately until a rate was attained that fused them into a tone, it was thought advisable to examine some physical instruments for the production of sound, which admit of adjustment to various rates above and below the lower limit of appreciation of tone.

It is well known that, with organ pipes 32 feet long, or with a monochord, the string of which by weighting has been made to vibrate below thirty times per second, sounds are heard having the character of an imperfectly fused rumble, in which the ear can distinctly detect the separate vibrations, but no distinctive tone.

Some exception might be taken to the evidence furnished by the use of pipes or strings as to the audibility of the separate waves on account of the possibility of harmonic sub-division of the column of air or string giving the octave as the chief harmonic. We, therefore, preferred to use tuning forks, in the case of which this objection does not obtain, as their vibrations are probably the most purely pendular, and the first harmonic of the fork is so much higher in pitch ($6\frac{1}{4}$ times the vibration frequency of the fundamental tone), as to be always readily recognisable if present. With a fork of 30 V.D. per second, a sound is produced which is audible through the air, but much more distinctly so by placing the base of the fork in contact with the head or by fixing the fork firmly in a block of wood which was auscultated by a binaural stethescope. The character of the sound remained the same when transmitted by these different methods. At this rate of vibration (thirty per second), the impression is just becoming continuous, but not sufficiently so as either to prevent the recognition of the separate vibrations or to produce a distinctive tone. By the use of brass weights, firmly clamped at different heights on the prongs of the fork, its rate of vibration was reduced to 28, 24, and 20 V.D. per second respectively, the rate being estimated by being recorded on a smoked surface moving at a known velocity. The effect of this reduction of rate is that, while the intensity of the sound becomes enfeebled, the separation of the constituent vibrations becomes more distinct and unmistakable. Thus the audibility of the fork becomes less and less as the rate is lowered, but the sound always preserves the same toneless and interrupted character. These effects of reducing the rate of

vibration ought not to take place if the audition depended upon adventitious vibrations set up in bodies in contact with the fork. But, if each vibration transmits a distinct movement to the ossicles of the ear, both these effects should be expected, and are easily explained. And the fact that, with the low rates of vibration, the waves in air are so long and the changes of pressure so gradual that the sound ceases to be appreciated through the air, while still distinctly audible through the bones of the head, also becomes clear. We may then conclude that when the rate of vibration is reduced to that at which the individual waves are no longer heard, the fork has become absolutely inaudible; while the power of differentiating tone is lost long before this limit has been reached.

It might be alleged that, although the fork gives rise to a series of pure pendular vibrations, these set up secondary vibrations of higher frequency in the membrane of the tympanum, or some medium through which the waves pass.

To test this point, we constructed a kind of phonautograph in imitation of the tympanic membrane. It consisted of a circular metal frame, over which a thin india-rubber membrane was stretched with very slight and adjustable tension. An extremely light lever, armed with a fine writing point, and poised on jewelled bearings, similar to the escapement of a watch, was brought into connexion with the centre of this membrane, so as to record its slightest movement on a smooth, lightly-smoked, moving surface. With this apparatus we were able to record the vibration of forks at the rate of 25 and 30 V.D. per second, when the fork was held about 2 or 3 cm. from the membrane, and the vibrations thus transmitted through the medium of the air. The tracing was made up of a series of regular undulations, entirely free from any indication either of over-tones or "self-tone" of the membrane. It appears, then, exceedingly improbable, if such a comparatively rough mechanism can transmit these slow toneless waves without exhibiting any tendency to persistent vibration of its own, that this should occur with the membrana tympani, the structure of which is adaptively modified so as to check, in particular, any such effect, and which is capable of differentiating such a wide range of tones with equal precision.

From the foregoing it would appear that not only is there no difficulty in understanding how these waves, which are too slow to produce a tone, are carried to the nerve, but also that the pure pendular vibrations of the tuning fork furnish conclusive evidence of the audibility of each single wave when reduced to a rate at which tone can no longer be discriminated.

In order to obtain single vibrations of the more rapid rate of higher notes (*i.e.*, vibrations of short durations isolated from the series causing the note) we employed a disk siren in which any number of

holes could be left open or closed at will. The wheel was turned by hand, about twenty-five rotations of the disk being the maximum attainable with the required regularity.

By leaving only one hole open, only one puff was made at each revolution. By varying the rate of rotation, waves of different duration, *i.e.*, corresponding to vibrations of different pitch, could be produced. No matter how the rate of revolution was increased, up to the maximum, twenty-five per second, only the single puffs could be heard; at first quite separate, then as a soft purr, and at the quickest rate like a kind of rapid patter.

With this siren, the sound caused by the single impulse is most distinct and clear, and its invariability in character with varying rate and varying blast of air particularly noticeable. The single puffs of a duration not exceeding $\frac{1}{1000}$ second were thus perfectly audible.

As the rates just referred to were estimated from the number of revolutions made by the driving wheel, we thought it advisable to control this method of measuring the length of the waves by ascertaining the note produced by the siren when all the holes were open while the disk revolved at the same rates. It was thus found that clear tones could be heard varying with the rate up to the maximal attainable limit, somewhat above the note C''', *i.e.*, 1056 per second.

II.

No doubt adventitious secondary oscillations followed the single puffs for a short period, and probably gave them character as well as making them more distinctly audible. Hence we wished to hear what character the tones would have if caused by a short series of these vibrations. We commenced with a number of holes open which, at our maximal rate, would have a duration of $\frac{1}{100}$ second. This gave a tone similar to, and quite as distinct as, that produced when all the holes were open. We then gradually reduced the number of holes used, and we found a fact which, as far as we know, has not been previously observed, and for which we were not prepared by the importance given by our physical friends to the necessity of *a series* of waves in the production of sounds. When only *two* holes remained open, the variation in tone following changes in rate was perfectly distinct, and a note even higher than C''', corresponding to 1056 V.D. could be heard perfectly. That is to say, a series made up of two puffs which lasted $\frac{1}{1056}$th of a second each, or less than $\frac{1}{500}$th of a second for the series, was capable of stimulating the terminals of the auditory nerve in such a way as to make the tone C''' readily recognisable.

When the single hole and the double hole were tried alternately, the result was striking. The pitch of the note caused by the short series of two puffs rose and fell with the increase or decrease of the

rate of rotation, while with the single hole the puff, though distinctly audible, remained the same monotonous sound, independent of sudden variations of the speed of the disk. The only change in character was that the intensity diminished with the lower rates of vibration.

From the foregoing, we have been led to the following conclusions:—

1. When sound is produced by a vibrating body, each individual wave of the series causing the tone stimulates the terminals of the auditory nerve. If the single vibrations are of such a nature as to be inaudible, no tone can be heard.

2. The individual vibrations can be heard when the rate of vibration is too slow for a distinctive tone to be appreciated.

3. The immediate succession of two waves, at rates of vibration above fifty per second, gives rise to a sensation of tone having the same pitch as that yielded by a prolonged series at the same rate.

That is to say, one can distinguish the tone produced when only two vibrations of a series reach the terminals of the auditory nerve.

4. Having worked for some time with rates of vibration near the limit of the lowest tones, we are forced to admit that we have not attained the skill (said to be attained by practice) of distinguishing small changes in pitch with rates of vibration below fifty per second; at least, in the case of pendular vibration, such as is produced by tuning forks or muscular contraction.

II. "On the Mechanism of the Closure of the Larynx. A Preliminary Communication." By T. P. ANDERSON STUART, M.D., Professor of Physiology, University of Sydney, N.S.W., Australia. Communicated by Professor E. A. SCHÄFER, F.R.S. Received December 10, 1891.

By a series of papers ending with that by myself with Dr. A. McCormick ('Journ. Anat. Physiol.,' January, 1892), it has been finally determined that the time-honoured doctrine of the closure of the larynx by a lid-like action of the epiglottis is quite untenable; but, so far as I am aware, no satisfactory account of how after all the larynx is closed voluntarily and reflexly has as yet been given. The determination of the positive side of the question was nevertheless the necessary sequel of the determination of the negative side; having settled how it *is not* closed, one naturally proceeded to enquire how the larynx *is* closed, since closed it at times must be, and that at its very entrance.

In the literature of the subject I find that there is considerable looseness of diction. "Closure of the larynx" may mean either "closure of the glottis," "closure of the vestibule," or "closure of

the superior aperture of the larynx." These are manifestly three very different things, and it is that of the superior aperture which was effected by the epiglottis according to the old doctrine. On account of this looseness of diction, the experiments of John Reid lose most of their value in this connexion.

Now when one reflects that closure of the glottis merely would still leave patent all that portion of the laryngeal cavity above the level of the glottis, the vestibule, a region exquisitely sensitive to mechanical irritation, one immediately perceives that if the epiglottis does not effect the closure, then some other agency must exist whereby the superior aperture of the larynx, its very entrance, is closed against the entrance of food particles during deglutition. It is quite immaterial where the closure takes place so far as closure of the larynx during forced efforts is concerned, but it is not immaterial where it is effected in deglutition, for the superior aperture, at least, must be closed, however much farther downwards the closure takes place. Were it to remain open, the vestibule of the larynx would be a regular funnel specially adapted, as it were, to take up particles of food. Such particles after the act of deglutition is over would need to be expelled by a violent expiration, a cough, or they would by their weight fall into, or by the force of the inspiratory air blast they would be drawn into, the lower passages. Now these things do not happen, so that *a priori* even, we may assume that the actual entrance to the larynx *is* closed, and experimentally I *have seen that it is* closed, while, on the other hand, Longet kept the margins of the glottis so apart that that aperture could not be closed, and yet the act of swallowing was carried out normally.

It may be well to give a straightforward account of how I have observed the larynx to be closed, and I shall then give details of experimental observations.

The observations were made on—

1. A man who had a large part of the side wall of the pharynx removed for carcinoma, without in any way interfering with the larynx. The man made a good recovery, and when feeding by the use of the stomach-tube was discontinued, he immediately swallowed as perfectly as he ever afterwards did, so that no education of the parts seemed necessary, and he continues to swallow about as well as he ever did, so that in function the parts are practically unimpaired. This man usually wears a rubber pad over the hole, but upon the removal of the same, that is, even with the hole open, one can watch with the unaided eye many of the phenomena of voluntary closure of the larynx, swallowing, coughing, singing, and so on. With the hole open a bolus of solid food is successfully swallowed, perhaps once out of three attempts; the other two times it will escape by the open hole.

2. A series of healthy persons examined laryngoscopically.
3. Frogs, Tortoises, Lizards, Birds, examined by simply opening the mouth and observing the top of the larynx, and the effects of stimulating certain of its muscles.
4. The Opossum, Cat, Dog, and Goat, anæsthetised with chloroform, and the laryngeal aperture examined through an incision in the middle line above the level of the epiglottis. Swallowing was observed as it occurred spontaneously or was evoked by irritating the pharyngeal mucous membrane. The relation of the tip of the epiglottis to the lower end of the incision formed an excellent guide as to whether or not the epiglottis moved. The complete closure of the larynx was tested by the stoppage of a current of air sucked down through the larynx.

In Man, and presumably in other animals with larynges of a like build, during respiration the arytenoid cartilages stand backwards and are rotated outwards. They, surmounted by the Santorinian cartilages and enveloped in the mucous membrane, are continually but somewhat irregularly advancing and retiring synchronously with the movements of expiration and inspiration. When they are backwards as in inspiration, the arytenoids, the posterior margin of the superior aperture of the larynx, lie against, and may indent, the posterior wall of the pharynx. In forced respiration these movements are exaggerated. The upward and backward direction continues more or less the direction of the plane of the lamina of the cricoid cartilage.

When the laryngeal entrance is to be closed, the arytenoid cartilages leave the posterior wall of the pharynx, are rotated and are moved bodily inwards, so as to bring their internal faces into contact, are inclined forwards, and glide forwards.

By the apposition of the arytenoids a mesial fissure makes its appearance, bounded posteriorly by the fold of mucosa containing the transverse arytenoid muscle and laterally by the arytenoids. The plane in which this fissure lies is, in Man in the erect posture, obliquely from before downwards and backwards. This fissure ends anteriorly in another fissure, which, however, is transverse; the two together constitute a triradiate fissure having the form of a squat T, the vertical limb being somewhat short, and the transverse limb rounded, owing to the pulling inwards, towards the middle line, of the margins of the epiglottis, so that the epiglottis thus forms a more marked hollow to receive the tips of the arytenoids.

The transverse fissure constituting the head of the T is bounded anteriorly by the epiglottis, and posteriorly by the ary-epiglottic folds. The ends of this fissure are closed by the junction of the ary-epiglottic folds with the margin of the epiglottis, and in the middle line posteriorly it receives the posterior or inter-arytenoid limb of the triradiate fissure.

Around the central point of this triradiate fissure are to be noted three prominences or thicknesses, viz., in front, and in the mesial plane, the cushion of the epiglottis more or less filling up the slight hollow left between the two prominences next to be mentioned, viz., slight thickenings at the point of flexion of the ary-epiglottic folds, where the lateral boundary of the antero-posterior limb of the fissure becomes continuous with the posterior boundary of the transverse portion; these are the tubercles of Wrisberg.

Having observed the appearances in the case of Dyason ('Jl. Anat. Physiol.,' *ut suprà*), I thought it advisable to get independent observations made by professed laryngologists, whom I had made acquainted with my observations. At my request, therefore, Dr. Barrett and Mr. Iredell, of Melbourne, kindly undertook to look into the matter, and here is what they say:—

Dr. Barrett.—"The appearances of the larynx as seen from above with the laryngoscope when closed during forcible expulsive efforts. A laryngoscopic examination was made in the cases of two young men with normal throats. They were directed to make expulsive efforts, as in defæcation. In both cases the appearances were similar. A view was first obtained during ordinary expiration, and then forcible expulsive efforts were made. The following changes occurred:—The cords and arytenoids approximated, and then the arytenoids moved forward until the mucous membrane and cornicula over them were firmly pressed against the epiglottis, at a point much higher than the attachment of the vocal cords. The epiglottis did not during the closure in any way alter its inclination, remaining vertical throughout. In one case, however, it became very much more curved round a vertical axis. It did not participate actively in the closure. The closed larynx may be said to show a triradiate figure like a shortened T, the vertical limb being much shortened, and representing the fissure between the arytenoids, and the crossed limb being somewhat curved."

Mr. Iredell.—"Undoubtedly during straining the posterior cartilages of the rim of the larynx come forward and appear to be about to form the figure you draw, but long before anything of the kind is perfect the muscular parts of the pharynx prevent all view. This is much more so during the act of swallowing. This much is clear: there is no sign of the epiglottis folding backwards and downwards, and as the act of swallowing proceeds this would appear to become more and more impossible. Yet there is an *appearance*—it may be only an appearance—of a tendency to move back the whole body of the tongue, carrying with it the epiglottis."*

* I believe that the difference in the amount seen by the two observers is due to the fact that Dr. Barrett probably used the cocaine spray, while Mr. Iredell did not. I remember that the former did use it in one case in my presence, while with the latter I did not speak of it.

If it be a simple voluntary closure of the laryngeal entrance, with or without expiratory effort, that is under observation, nothing further is to be noted. When the entrance is opened the arytenoids leave the epiglottis and then each other, are rotated outwards and backwards, move bodily outwards, glide backwards, and thus again assume the position they occupy in respiration.

If, however, the entrance is to be closed as a part of the act of swallowing, then, of course, the well known movement of the entire larynx upwards and forwards ensues, and the tips of the arytenoids are seen to be jammed firmly against the epiglottis. This is due partly to the thyro-arytenoid vigorously rotating the arytenoids inwards, and pulling them downwards and forwards, so that their tips come into contact with the base of the epiglottis; partly, however, it is due to the elevators of the larynx pulling the larynx upwards and forwards against the base of the tongue. In this position of the larynx the epiglottis lies between the rest of the larynx and the tongue and is firmly applied to both, is in fact compressed between them. It is, however, clear that if the epiglottis were not there the laryngeal entrance would still be closed, partly by the gathering up of its margins as above described, and partly by its direct contact with the base of the tongue, there being now no epiglottis to intervene.

The behaviour of the distal or apical portion of the epiglottis at this stage is not the same in all animals. In the Dog, for instance, the epiglottis is extremely flexible, and comparatively short, and is thus easily engaged between the tongue and the larynx. In the Dog, therefore, the distal portion of the epiglottis *has the appearance* of closing the laryngeal orifice in the lid-like way usually described as general in its application; but how little this is essential is at once evident when we remember the little or no inconvenience following its complete removal. In animals such as the Goat, which, like Man, has the distal portion of the epiglottis long and stiff, quite another picture is presented during this stage of the act of swallowing. It is only the base of the epiglottis which is engaged between the base of the tongue and the larynx; the distal portion does not fold down as a lid, but is applied to the most posterior part of the back of the tongue, so that the hollow laryngeal surface of the epiglottis continues backwards the surface over which the bolus glides from the tongue.

This, I think, may indicate the function of the hyo-epiglottic muscle about which there has been so much doubt. May it not serve to pull the epiglottis towards the hyoid bone during the act of deglutition, so that the epiglottis would be drawn upwards and forwards *with the larynx?* The hyo-epiglottic muscle would then stand in the same relation to the epiglottic cartilage as the hyo-thyroid muscle does to the thyroid cartilage. In this way the

epiglottis would lie on the tongue's surface, and be firmly pulled against it, so that the bolus would have less chance of getting between the epiglottis and the tongue in its passage downwards. Not all animals have this muscle, but then differences in the arrangements of the other parts may account for this. My experiments show that writers have hitherto taken too little notice of the differences in the anatomy of the larynx in different animals. These differences are very considerable.

I think we can generalise by saying that the closure of the larynx is invariably effected by contact of the arytenoids with each other and then contact of the two together with some part of the anterior wall of the laryngeal cavity, *but how this latter contact is effected* varies with the anatomical arrangements of the parts.

The extent of the contact of arytenoid with arytenoid varies. It may be (1) by the entire internal faces of the cartilages, (2) by an area along the anterior margin only, in which case the mucosa over it may be very thin and the whitish cartilage may show through, as in the Koala, or (3) the cartilages, though brought together, cannot, owing to their form, close the respiratory glottis (Milne Edwards).

We may, I think, divide* arytenoids into relatively large and relatively small. Then the former into relatively narrow and relatively broad. Thus we get three groups of arytenoid cartilages, viz., (1) high and narrow, (2) high and broad, (3) small.

In the high and narrow group the arytenoids fold over into contact with the front wall of the laryngeal cavity: the base of the epiglottis as I have described in Man and the Goat.

In the high and broad group, including many of the Marsupials I have examined, the arytenoids move more bodily forwards into contact with the base of the epiglottis, or at least the front of the vestibule.

In the group of small arytenoids, neither folding nor movement bodily forwards would suffice to effect the contact, and here the lower part of the epiglottis is permanently bent backwards, so that the wall of the upper and front part of the laryngeal cavity forms a little hood over the vocal cords, about the posterior margin of which hood the arytenoid contact takes place. In this case, therefore, the epiglottic base has, as it were (permanently), gone to meet the (small) arytenoids, which thus are able to effect the contact with a minimum of movement.†

In all cases I attach the greatest importance in effecting the laryngeal closure to the contact of the larynx as a whole with the

* Provisionally.

† On looking over the preparations of the larynx in the Hunterian Museum, I was struck with the frequency of this hooded condition of the epiglottis; it seems almost the rule.

base of the tongue, with or without the intervention of the epiglottic base. The movements of the arytenoids constitute a gathering up of the back and side boundaries, while its front boundary is virtually gathered up by its remaining stationary against the base of the tongue while the whole larynx moves forward. This is why swallowing is so often but little affected by the loss of the epiglottis by disease, accident, or experiment. Even loss of the arytenoids by disease does not seem to necessarily cause difficulties in deglutition.

I think it more than probable that there are differences between various species, and even between individuals of the same species, as to the importance of the part played by the tongue in closing the larynx. This seems to follow from the very various anatomical dispositions of the parts, and may account for much of the difference in the symptoms and signs of a particular laryngeal lesion in different individuals. In one case a lesion of the arytenoids or ary-epiglottic folds is not followed by difficulties of swallowing, while apparently the same, or even a less, degree of the same lesion in another case is followed by almost total inability to swallow, at all events, liquid food. For instance, in John Reid's experiments of cutting all the four laryngeal nerves in four Rabbits, two continued to take milk and two refused it. Two Dogs similarly treated continued to take both solids and liquids. In none of these cases was food found in the air passages after death. Reid therefore concludes that "the epiglottis . . . can prevent the ingress of food into the larynx when the movements of all the muscles which diminish the size of the glottis (*sic*) have been suspended by section of the laryngeal nerves." ("An Experimental Enquiry into the Function of the Eighth Pair of Nerves," 'Edinburgh Medical and Surgical Journal,' January, 1838.)

This same forward and upward movement of the larynx brings the lamina of the cricoid cartilage away from the back wall of the pharynx, and so provides the room necessary for the passage of the bolus. When the arytenoids are in position forwards, their highest points are their margins bounding the mesial fissure. This fissure thus traverses, as it were, a little ridge from before backwards, and from this ridge the top of the closed larynx slopes downwards at the sides and posteriorly, but especially at the sides.

Thus, when the laryngeal entrance is closed as for deglutition there is a fairly even surface for the bolus to glide over, from the laryngeal face of the epiglottis to the posterior surface of the arytenoids and lamina of the cricoid, and so into the gullet.

According to this account of the closure of the laryngeal entrance, the arytenoids enclosed in their mucosa—an arytenoid valve or flap—take the place of the epiglottis according to the old account, which is now all but universally discredited.

I have no doubt that the apparent fitness of the epiglottis as a lid

to cover the voice-box has given a longer life to the time-honoured doctrine of the functions of the epiglottis than it would otherwise have had; it seemed so beautifully fitted for its office that until recently it did not occur to any one to question it. Upon consideration, however, of the part played by the arytenoid valve according to my account, it appears to be much more beautiful than the part played by the epiglottis in the old account of the closure of the larynx, for, in addition to the merit of being demonstrably true, it co-ordinates and explains many observations hitherto without any connecting link or explanation, as will be set forth later on. In the meantime, however, I may point out how this arytenoid valve stands at the parting of the ways downwards out of the pharynx: when it stands backwards the food-passage is closed and the air-passage is open, when it lies forwards the air-passage is closed and the food-passage is open. Thus it can make, as it were, a funnel forwards into the air-tube or a funnel backwards into the food-tube. In short, and to use a particularly Australian illustration, it stands like the little movable gate by which sheep are drafted out of the common yard into separate pens—it drafts the air forwards and the food backwards.

This use of the term funnel is fully justifiable. In ordinary respiration, especially during inspiration, since the top of the arytenoid flap lies against—may even indent—the posterior pharyngeal wall, the way to the gullet is stopped, while the vestibule of the larynx is wide and patent, and, of course, is wide at the entrance and narrows downwards to the glottis. In this condition the shape of the entrance is apparently five-sided, though really there is a sixth, but comparatively small, side in the middle line posteriorly, where the transverse arytenoid muscle is. The anterior side is formed by the epiglottis. The lateral margins consist each of an anterior moiety formed by the ary-epiglottic fold, and of a posterior moiety containing the arytenoid tips and Santorinian cartilages. When the larynx of the Goat is exposed, this anterior funnel is peculiarly striking during forced inspiration, and the superior margin of it is almost circular, the violent outward and backward movement of the arytenoid tips pulling backwards the margin of the epiglottis, and so rounding off the anterior angles, and the flexible tips of the arytenoid and Santorinian cartilages yielding to the pull of the ary-epiglottic folds, and so rounding off the lateral angles.

The posterior funnel, though less striking, is hardly less real than the anterior one. It really exists only during the act of swallowing, and then also its anterior wall is composite and somewhat irregular. The anterior wall is formed above by the epiglottis, in the middle by the back of the arytenoid flap, and below by the back of the lamina of the cricoid. Now, while the epiglottis is always more or

less in position, the arytenoid portion is only there during closure of the laryngeal aperture, and the lamina of the cricoid is heaved forwards only during deglutition. Thus only during deglutition does the lamina of the cricoid form the inferior part of the anterior wall of a funnel.

Very much the same condition of parts is seen in *Manatus* (Waldeyer, 'Sitzungsb. der König. Preuss. Akad.,' Berlin, 1886). "Here one cannot speak of a bifurcation of the food channel as if it went to the right and left of the epiglottis, for, in *Manatus*, even small quantities of fluids must reach the œsophagus by passing straight over the larynx. Since, however, the laryngeal entrance is firmly closed in the way I have described, and so makes a surface gently inclined backwards, the entrance of liquids and solid foods is efficiently prevented."

The two funnels then, the air-funnel and the food-funnel, are alternately conditioned, the former solely, the latter largely, by the movement of the arytenoid flap backwards and forwards respectively.

In passing and in this connexion, one may point out a part played by the lamina of the cricoid. When the arytenoids move forwards their vocal processes move towards each other and at the same time downwards, so that the plane of the glottis comes to lie lower posteriorly. Anteriorly, of course, the plane is fixed by the attachment of the true cords to the thyroid cartilage. Thus the special vocal apparatus during deglutition lies deep down within the laryngeal cavity, and by the lamina of the cricoid is protected from the pressure of the bolus, pulled forwards, as it is, by the muscular slings of the inferior constrictor of the pharynx.

If we think of the old descriptions of the closure of the laryngeal entrance by the folding back of the epiglottis, we see at once that there would be a most awkward angle round which the bolus would have to travel just as it entered the gullet. This angle would be formed above by the tip of the epiglottis, and below by the posterior margin of the laryngeal entrance, *i.e.*, the tip of the arytenoid flap. A similar angle would exist in the case of all animals the plane of the entrance to whose larynx crossed the axis of movement of the descending bolus, the angle being the more marked the more nearly at a right angle this plane crossed this axis, and it is partly to get rid of this angle that the folding forwards of this arytenoid flap takes place. In animals, on the other hand, where the plane and the axis are parallel there is no angle and there is no folding forward of the arytenoid—merely an outwards and inwards movement, an opening and shutting of the lozenge-shaped entrance, as in the case of the Tortoise, Lizard, Frog, Snake, &c. In addition to the above angle, there would be also an inconvenient open angle between the tongue and the epiglottis, in which food particles would be most likely to lodge, and

upon the resilience of the epiglottis, being engaged between it and the tongue, they would give rise to irritation. As a matter of fact, it does sometimes happen that food gets between epiglottis and tongue, but it would surely often happen if the doctrine of the lid-like action of the epiglottis were true.

By my account of the normal closure of the larynx many further points in the anatomy of the larynx and many observations as to its physiology now receive an explanation. Some writers have come near to the truth, and of all Luschka has come nearest, many passages in his writing showing that he has just failed to grasp the whole meaning of what he describes.

The External Thyro-arytenoid Muscle.—The direction of its fibres is in the main from before, backwards and upwards; this is clearly the direction most suited to the folding forwards of the arytenoid cartilage. Thus the origin of the muscle is in front, at the thyroid cartilage as the more fixed end. The insertion of the muscle extends in the vertical direction, well nigh throughout the entire length of the arytenoid cartilage, so that its superior fibres have a great mechanical advantage in inclining the cartilage forwards, while its inferior fibres especially pull it forwards at its base. Owing to its insertion into the outer surface of the arytenoid cartilage, it tends powerfully to rotate the cartilage so inwards that the internal faces of the cartilages come into apposition. Thus the muscle brings about three of the movements of the arytenoid cartilages that close the larynx, viz., rotation inwards, inclination forwards, and gliding forwards at the crico-arytenoid joint. These movements have been seen experimentally upon electrical stimulation in Man.* The fourth movement is the apposition of the arytenoids by the *arytenoideus transversus.*

The muscle thus with the transverse arytenoid forms a sphincter for the larynx.†

Having this function of the muscle in mind, one at once understands why it is such a large muscle, why it extends vertically so far beyond the level of the true cords, and in short why as viewed in a vertical transverse section of the larynx it seems to have so little relation to the true cords: the fact is *it* is (with the transverse arytenoid muscle) *the* true "sphincter vestibuli,"‡ for it rotates inwards the

* Von Ziemssen, 'Die Electricität in der Medizin.'

† The term "constrictor vestibuli" has already been employed, but applied to the ary-epiglottic muscle, by Luschka. This manifestly is incorrect, for this muscle could not constrict the vestibule, *i.e.*, down to the level of the lower border of the superior cords, however much it might constrict the entrance or *aditus laryngis.*

‡ The term "sphincter" is applied by Henle to the aggregate of the thyro-arytenoids, arytenoid and ary-epiglottic muscles, but his notion of the action of this muscular mass is that the arytenoid pulls the arytenoid cartilages together, while the fibres that have in the relaxed condition a bend with the concavity inwards

anterior margins of the arytenoid cartilages, and thus brings together the sides of the vestibule, and then it inclines the arytenoids so as to encroach on the space from above downwards, and from behind forwards—the three muscles, that is, the two thyro-arytenoids and the transverse arytenoid, now grasp the vestibule, so to speak, obliterate its space, and thus close the larynx. With them act the ary-epiglottici so as to close the very entrance.

In certain animals there is no elastic tissue in what would correspond to the true cord (Bland Sutton, 'Journal of Anatomy and Physiology,' "Nature of Ligaments"), which thus is entirely of muscle enclosed in mucous membrane. This seems to indicate the primary importance of the muscle as a muscle rather than as a vibrating cord, a view also expressed by Bland Sutton upon purely anatomical grounds. May it not be simply that the elastic property is secured for the vibrating tissue at a physiologically cheaper rate through fibrous than through muscular tissue?

Nothing is said here of the feeble *thyro-arytænoideus internus:* this, probably, is the proper muscle of the vocal cords, and in any case, from its feeble development and its attachments, it cannot have much influence in closing the larynx.

This description of the mode of closure of the larynx meets the case during both inspiration and expiration. In closure during inspiration the closure is absolute and, after the muscles have once brought the parts into position, is entirely mechanical. This one can easily verify by sucking air down through either the dead or the living larynx. The margins of the aperture come together and remain pressed together by atmospheric pressure. Against expiration the resistance is entirely muscular, for the margins of the aperture open outwards, and air can always be blown upwards so as to force open the closed larynx. The thyro-arytenoid and transverse arytenoid appear to me to be quite adequately developed to resist pressure to a high degree, because, in the first place, these muscles are really not so small as one is accustomed to think they are; and, in the second place, the surface which has to bear pressure in the closed larynx is not extensive.

In such larynges as the Lizard's, the muscle simply rotates the cartilage to close the lozenge-shaped aperture which corresponds to the glottis of Man.

The Arytenoid Muscle.—This muscle must be divided into (1) the two oblique portions, which are really continuations of the ary-epiglottic muscle, and are dealt with under that heading; and (2) the transverse portion. The transverse portion alone is now under consideration. Owing to the articular surface of the arytenoid car-

straighten themselves, and so bring the side walls of the entrance together. This, of course, is quite a different thing from my account.

tilage being on the under surface of the muscular process, which is projected backwards from the cartilage, the surfaces of attachment of the muscle lie in front of the joint, *i.e.*, in front of the fulcrum, so that the muscle powerfully rotates the cartilages inwards, draws them together, apposes their internal surfaces, and pulls them powerfully together. Thus the muscle acts with the thyro-arytenoids as a sphincter of the larynx. The muscle likewise forms an elastic ligament between the two cartilages, fibrous tissues being clearly unsuitable. White fibrous tissues would imply fixity of interval, and elastic tissues would need in certain positions to be stretched by considerable muscular force, while in other positions they would be lax. The muscle likewise forms part of the surface over which the bolus glides in deglutition, but at this time it is in contraction, and therefore firm; and, lastly, it closes in the triangular space which must always remain in front of the lamina of the cricoid, even when the arytenoids are applied to one another by their inner faces.

The Ary-epiglottic Muscle, including the Oblique Portion of the Arytenoid Muscle.—Its function, apparently, is to make tense the ary-epiglottic folds of mucous membrane, part of the immediate boundaries of the entrance. Thus the entrance is bounded during deglutition either by cartilage or by tense muscle, so that the bolus has no chance of entering. The tension is manifest from the more marked backward curvature of the lateral edge of the epiglottis; thus the concavity of the epiglottis becomes more marked, forms, indeed, a deeper groove to receive the tips of the arytenoids during deglutition. On this account the transverse limb of the fissure, or head of the T, is markedly concave backwards.

That the muscle is prolonged over the back of the upper part of the cartilage of its own side to the base of the cartilage of the opposite side is necessary. It thus tends to brace together the two cartilages, whereas, if it only went to the cartilage of its own side, it would tend to separate the cartilages, and thus to destroy the effectiveness of the closure of the orifice.

A subsidiary effect is to help the thyro-arytenoid in rotating inwards and pulling forwards the arytenoids; to this extent therefore, but only to this extent, is the name "constrictor vestibuli" (Luschka) justifiable.

The Lateral Crico-arytenoid Muscle.—Besides rotating the arytenoid cartilage, this muscle must also help in tilting the whole cartilage forwards by pulling the base forwards so that the posterior part of the articular surface comes to rest on the cricoid. Both this muscle and the posterior crico-arytenoid being inserted at the base of the cartilage, therefore very close to the axis of rotation, secure a comparatively large movement with but little actual shortening of the muscles.

In most Mammals the larynx is usually open, and is only shut when some temporary occasion arises. In some Mammals, such as the Porpoise and Dugong (Owen), Grampus, White Whale, Dolphin, &c., the larynx, on the other hand, appears to be usually shut, and here the ⊤-shaped fissure which I have described is quite evident. Is not the usual condition in these Mammals an indication of what is most likely to be the temporary mode of closure in the others?

Birds are extremely instructive in this connexion. Here the vocal function is entirely removed from the larynx, so that the larynx has for its sole office the guarding of the entrance to the trachea. Inspection and experiment show the entrance to be closed by the arytenoid cartilages, or bones, and the thyro-arytenoid muscles. Since this is their function in Birds (and the same applies to Tortoises, Lizards, Reptiles, Frogs, &c.), is it not all the more likely to be at least a function in Mammals? Bland Sutton (*loc. cit.*) in this connexion, and upon other grounds, believes that "the original function of the vocal cord is to protect the air passages, speech being a superadded function." Closure of the larynx is the one never failing office of the larynx, and the arytenoid cartilages and their muscles are the only never failing structures; epiglottis, false cords, true cords, as such, and ventricles may all be absent. Does not this indicate some connexion?

The ⊤-shaped fissure seems to depend upon the presence of an epiglottis; where that is present, it keeps the anterior end of the fissure wide, makes, in short, the transverse head of the ⊤. Where there is no epiglottis and nothing to take its place, the fissure is purely antero-posterior, so that at the vertical limb of the ⊤ is the more primitive, and by making a succession of transverse sections of the closed larynx, the head of the ⊤ gradually narrows with the narrowing of the epiglottis, so that at the level of the glottis the vertical, *i.e.*, antero-posterior, limb alone remains even in Man. It will be observed that in cutting away the higher parts of the larynx, one has removed that part which is peculiar to Man and other animals having larynges of a similar build. Only the bases of the arytenoid cartilages and the attached true cords remain. This latter level thus corresponds to the opening of, say the Frog or Tortoise, and the shape of the opening is practically the same, a lozenge.

The heightening of the arytenoids in Man appears to give these two advantages: 1st, it permits of the entrance to the air passages having the funnel shape which favours the entrance of air; 2nd, it withdraws the vocal apparatus from the vicinity of the very entrance, so that it is the better protected.

Closure of the entrance to the larynx by "tight closing up of the arytenoid cartilages and ary-epiglottic folds" was observed by the laryngoscope (Bruns, 'Arch. f. Path. Anat.,' vol. 43, 1868, p. 135,

2 A 2

quoted by Luschka) in the case of a girl who had lost the epiglottis by ulceration, but Luschka concludes from a consideration of other cases that this was a mere accommodation gradually effected by the muscles "by prolonged practice acquiring the necessary strength to mutually approximate the side walls" of the vestibule. In the case reported by myself with McCormick already referred to, the patient, *immediately* upon the cessation of feeding by the stomach-tube, could swallow as well as he did later on, and to all intents and purposes as well as he could before the operation, and in him we were able to see by merely looking through the hole in the side of the neck, that the laryngeal closure was accompanied by the T-shaped fissure. Here, therefore, there was no "prolonged practice"; there was no practice at all, and the same remark applies to the cases already quoted as having been examined for me by Barrett. The fissure is also shown in Czermak's classical work, but the formation of the head of the T is misinterpreted; it is said to be by the backward movement of the epiglottis, instead of by the forward movement of the arytenoids.

Short of complete closure, the arytenoid flap comes forward so that it is approximated to the front wall of the laryngeal cavity, therefore, in Man, to the epiglottis, and if air be now expired, the characteristic sound of straining is produced by the vibration of the margins of the entrance to the air passages, not of the vocal cords merely.

Lister (Holmes 'Surgery,' Article on "Anæsthetics") ascertained laryngoscopically that true chloroform stertor is produced by vibration "of the posterior part of the arytæno-epiglottidean folds, which are carried forwards to touch the base of the epiglottis during the stertorous breathing, and are placed in still further apposition with it when the obstruction becomes complete." Thus, Lister saw "an antero-posterior co-aptation of the structures of the laryngeal aperture at a somewhat deeper level; without any change in the position or form of the epiglottis, towards which the folds of the mucous membrane above the apices of the arytenoid cartilages are carried forwards till they are in contact with its base. This is seen in coughing and also in laryngeal stertor." Nevertheless, the closure by the epiglottis is the mode of closure Lister adopted for deglutition.

In certain lesions of the true cords, the gruff voice is, as Sir Joseph Lister communicated to me orally, to be ascribed to a vibration of the ary-epiglottic folds, a suggestion with which I entirely agree. I believe also that in gargling, the larynx is almost closed, and the air issues under pressure from the narrow fissure I have described.

In the Kangaroo apparently a great extent of the arytenoids is exposed to the friction of the passing bolus, and along the ridge of the apposed arytenoids, where the friction takes place, the cartilage shows through, and the mucosa is not movable. In the human larynx this

arytenoid ridge contains the antero-posterior limb of the ┬-fissure, but it is much shorter and less prominent than in the Kangaroo.

The elastic nature of the tips of the arytenoids and the Santorinian cartilages admirably fits them for gliding into position down the front of the laryngeal cavity. In this respect also, the form of the Santorinian cartilages, convex forwards in the closed aperture, helps, and the cushion of the epiglottis, when present, corresponds to the interval between them. In the open aperture, the tips of the cornicula are directed inwards and backwards, but this merely brings them parallel when the arytenoids are in the closed position. The elastic nature of all these cartilages enables them to fit each other perfectly when brought into apposition, and to recover their shape when the pressure is removed, and the pad of fat in front of the epiglottis helps in this connexion. Even in the dead subject, these phenomena can be observed more or less by simply pressing the arytenoids forwards—the closure is perfect without the epiglottis ever moving.

In animals with the cornicula more highly developed than they are in Man and the Goat, this account does not apply. In the Dog, for instance, they are long and pointed, and are pushed aside out of the mesial plane when the larynx is closed.

The superior or false vocal cords, as the arytenoids are rotated inwards, of course, come into apposition with each other, since posteriorly they are attached to the anterior margins of the arytenoids and anteriorly they are attached together to the re-entrant angle of the thyroid cartilage. As the arytenoid cartilages move forwards, the superior cords are shortened from before backwards, and possibly this may account for the large amount of elastic tissue in their structure, and it may possibly be the office of the bundles of striped muscular fibres, which have been described as radiating into the false cords from various muscles of the region, to perform a similar function, that of taking up the slack of the shortened false cord. As the action of the thyro-arytenoid is continued, the soft substance of these cords will tend to be squeezed both upwards into contact with the advancing arytenoids, and downwards to encroach on the ventricle. At the same time, the sacculi will be compressed from the sides by the thyro-arytenoids and from behind by the arytenoid cartilages. This may be the reason why the sacculus is over only the anterior part of the ventricle, for here it is out of the way of the advancing arytenoid, and any secretion which might be squeezed out of it would immediately pervade the whole length of the ventricle, now narrowed by the encroachment of the false cords, and so would lubricate the whole length of the true cord, although the sacculus is over the anterior part only. The fact that we often swallow when the cords are dry, as in hoarseness after much speaking, supports this idea, for the saliva swallowed cannot possibly affect the cords; the

act of swallowing, however, could, by expressing the saccular secretion, and distributing this secretion as well as the copious secretion of the rest of this region over the true cords.

The Crico-arytenoid Joint.—The relatively great antero-posterior diameter, the ovoid form, and plane surface of the arytenoid articular surface, in the light of this description of the mode of closure of the glottis are quite comprehensible.

As the vocal process of the arytenoid with its attached true vocal cord sinks into the larynx, it moves nearly in a circle, of which the cord is the radius, and the cartilage glides forwards on the cricoid, so that the cord's tension remains fairly equable. As a matter of fact, however, the tension of the cord in the sunk position is greater than before, so that the mere gliding of the arytenoid does not wholly compensate: the tilting forward of the whole cartilage is relatively great, so that the vocal process is carried relatively far backwards and so the cord is put upon the stretch.

When the larynx is open the narrow end of the ovoid rests on the cricoid, but in the closed larynx it is the broad end of the ovoid. In the open larynx, stability is not of moment; rather is mobility important, for there is no pressure to be resisted and in swallowing it is of importance to get the cartilages quickly into position, and, moreover, we saw that the arytenoids are in continual movement even in tranquil respiration. In the closed larynx, on the other hand, stability is of the utmost moment, and then, not only is the broad end of the ovoid on the cricoid, but possibly the little intra-articular fibro-cartilage attached to the capsular ligament of the articulation, and projecting into the joint cavity from the posterior blunt circumference of the arytenoid articular surface (Luschka), comes into play to increase the surface upon which the arytenoid rests: only along a line can there be contact of the cricoid cylinder with the arytenoid plane, but the fibro-cartilage probably increases this surface of contact so that the arytenoid rests on the cricoid in front, and on the intra-articular cartilage behind. The fibro-cartilage filling up the angle of the joint posteriorly would play also the part of a sort of patella protecting this extremely important joint from any mechanical violence from the passing bolus.

A further advantage of the gliding forwards of the arytenoid is that, while in the open larynx the muscular process projects somewhat behind the plane of the cricoid lamina, in the closed larynx it does not, so that there is no impediment to the descending bolus, and less chance of damage to the cartilage and joint.

The long axis of the arytenoid articular surface is from behind inwards and forwards, and this is the direction in which the base glides.

The arytenoid cartilages of course glide laterally also, for the

cricoid articular surface is about half as long again as the arytenoid is broad—in the open larynx the arytenoids rest on the outer end of the cricoid surface, in the closed larynx on the inner end, so that they now lie closely apposed. Thus the action of the three true sphincter or constrictor muscles brings the arytenoids exactly into the position described in the closed larynx; the *thyro-arytenoidei* rotate the arytenoid cartilages inwards and pull them forwards, the *arytenoideus transversus* pulls them together. The narrowing of the lamina of the cricoid as we ascend has an important influence in permitting the arytenoids to approach each other bodily, though a triangular space, the base of which corresponds to the width of the cricoid lamina, must always remain filled by the *arytenoideus.*

The epithelium on the inner faces of the arytenoid cartilages is stratified squamous (Davis, quoted by Klein), and this is accounted for by the pressure between the two comparatively hard and resistant arytenoids. Covering the greater part of the side walls of the vestibule it is ciliated columnar, but there being no rubbing of surface upon surface, the mucosa being soft, and the surface being covered by a thick layer of mucus, the cilia are not damaged by the compression of the vestibule in deglutition. The epithelium for a little way within the margin is stratified squamous; here, however, one can imagine that some friction takes place during the movements of the parts.

The shape of the epiglottis fits in with my account of the act of deglutition: it lies on the posterior part of the dorsum of the tongue, but it would certainly not fit the top of the larynx did it fold over it as in the current description, for there is no relation either in size or form, nor is there any pattern on its laryngeal surface as if it were often applied to the laryngeal aperture. And, as Howes writes (' Jl. Anat. Physiol.,' 1889, p. 271), " In many Quadrupedal Mammals the parts are so arranged that the posterior border of the velum appears to overlie the epiglottis, abutting against the ventral laryngeal wall. In such a case, did the epiglottis merely function, during deglutition, as a lid, the effects of its displacement would be simply that of forcing it back upon the velum palati. A flapping action in deglutition, as ordinarily understood, could manifestly only be possible in forms in which the velum stops short of the epiglottis."

III. "Additional Observations on the Development of *Apteryx*." By T. JEFFERY PARKER, B.Sc., F.R.S., Professor of Biology in the University of Otago, Dunedin, New Zealand. Received December 15, 1891.

(Abstract.)

The paper is founded upon the study of three embryos of *Apteryx australis* obtained since the author's former communication on this subject was written.

The youngest (stage E′) is intermediate between E and F of the former paper, the next (F′) between F and G, the most advanced (G′) between G and H.

In E′ the characteristic form of the beak has already appeared.

In F′ the pollex is unusually large, giving the fore-limb the normal characteristics of an embryo wing.

Several important additions and corrections are made to the former account of the skull, especially with regard to the pre-sphenoid region, the basi-cranial fontanelles, and the relations between the trabecular and para-chordal regions.

The account of the shoulder-girdle is amended. In *Apteryx oweni* the coracoid region is solid, and no pro-coracoid appears ever to be formed: in *A. australis* a ligamentous pro-coracoid is present at a comparatively early period (stage F′ and perhaps E′).

An intermedium is present in the carpus in all three specimens in addition to the elements previously described.

The brain in stage G′ is interesting, as being at what may be called the critical stage; the cerebellum is fully developed, and the optic lobes have attained the maximum proportional size and are lateral in position. In all essential respects the brain of this embryo is typically avian.

IV. "On a Differential Electrostatic Method of measuring High Electrical Resistances." By Major CARDEW, R.E. Communicated by Sir WILLIAM THOMSON, D.C.L., P.R.S. Received January 6, 1892.

The following method has been found useful for determining the relative value in insulating quality of small samples of materials, the insulation resistance of short pieces of cable, and other very high resistances.

The arrangement is also suitable for continuously indicating the position on any electrical circuit, worked at a high pressure, of the

resultant fault or point of zero potential; and for measuring the insulation of the circuit while the pressure is on.

Connections.—The method consists in connecting the quadrants of an ordinary quadrant electrometer to the terminals of a source of fairly high E.M.F., while the aluminium vane or needle is connected to earth.

The resistance to be determined is connected to one side of this arrangement, and a variable resistance of the same order of magnitude to the other side, the free ends of each being connected to earth.

The centre of the battery, or other source of E.M.F., is then earthed for a short time, bringing the needle to the zero reading, and, after the removal of this earth connection, the needle will travel to one side or the other, unless the resistance to the passage of electricity from each pole to earth is exactly equal, in which case the needle will remain permanently at zero.

By observing the motion and varying the comparison resistance accordingly, this balance is soon arrived at, if within the range of variation provided.

The arrangement is shown in the figure, where B is the battery or other source of E.M.F., Q the quadrants, N the needle, X the unknown resistance, and R the variable resistance. The earth contacts are shown by E.

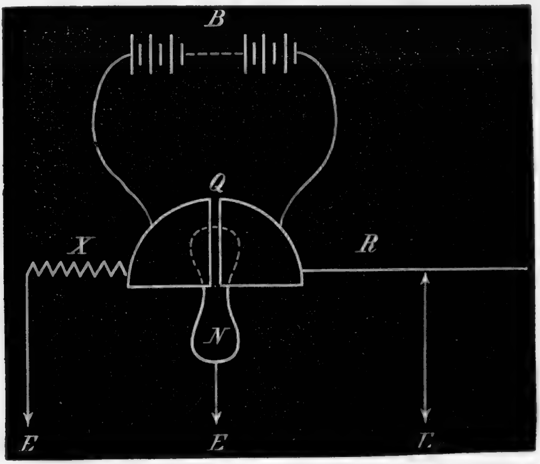


Diagram of connections for electrostatic balance for very high resistances. The opposite quadrants of electrometer are not shown.

Principle.—The method depends upon the well-known fact that every source of electricity produces equal quantities of what are

commonly called the two kinds of electricity in any time, however extended, and at any instant at an equal rate.

I have recently drawn attention to this law as determining the potentials from earth of the two sides of any system of electric supply (*vide* my paper read on the 23rd April, 1891, at the Institution of Electrical Engineers).

If we conceive, therefore, a perfectly insulated voltaic battery, the potentials of the terminals of this source from earth would be determined by momentarily connecting any one of the metal plates with the earth. Under such conditions, the smallest leakage from either pole to earth through a resistance amounting to many millions of megohms, if unbalanced by any leakage from the other pole, must rapidly reduce the potential of the imperfectly insulated pole to zero.

The only limit, therefore, to the sensibility of the method is the imperfection of the insulation of the measuring apparatus, and this insulation, with proper precautions, can be easily maintained at a value exceeding 1,000,000 megohms.

Sources of E.M.F.—A very convenient source of E.M.F. for the purpose is the arrangement of small zinc-copper couples in series, moistened by dipping the whole into a pan containing acidulated water, which is in use at the Physical Laboratory at Glasgow University.

Four hundred such couples are usually arranged on an ebonite support, and the sensibility with this number is ample.*

A still better source, when alternating currents are available, is a special form of transformer, the secondary coil being suspended in air by a silk cord.

The highest insulation can thus be secured. But when the resistance to be balanced possesses appreciable capacity, the use of an alternating E.M.F. is unsuitable, on account of the masking of the effect of the leakage current proper by the capacity current.

Comparison Resistances.—The variable resistance is, most conveniently, some material of uniform cross section, so that its resistance varies as the length put in circuit. Reels of white silk, cotton, thread, and string are very suitable, and with a few such simple materials, balances can be obtained through a great range of value, although no multiplying or dividing power is possible. Thus, a white embroidery silk has been found to have a resistance of approximately 250,000 megohms per inch; a green thread, partly silk and partly cotton, 10,000 megohms per inch in a dry atmosphere;

* The mahogany legs supplied with this battery should be replaced by grooved ebonite legs, to improve insulation, and it is also of advantage to insert under each leg a piece of sealing-wax. The couples require to be taken out and cleaned occasionally; if allowed to get dirty, the E.M.F. becomes low.

an ordinary measuring tape, 1400 megohms per inch, &c., down to a piece of wet tape, which gave 64,000 ohms per inch.

These resistances are, to some extent, affected by the degree of humidity of the air, but, when necessary, they can be rapidly standardised with sufficient accuracy by determining one of the lowest by the usual method; or, as a check, when time allows, a highly insulated condenser can be shunted by a length of silk, and the loss of charge, in a given time, measured.

Unsymmetrical Insulation of Apparatus.—If, from any cause which cannot be discovered or removed, the insulation resistance of the apparatus is unsymmetrical, indicated by the needle taking up a false zero when connection is made between the battery and quadrants, symmetry can be always secured by connecting a length of silk, found by trial, between the more highly insulated pole and earth.

Limits of Accuracy.—The accuracy attainable by this method depends on the sensibility of the electrometer and the potential difference employed.

With an ordinary suspension, however, it has been found that with a battery giving about 350 volts the difference in reading between that with the centre of the battery earthed and that with the earth connection made at 1 volt from the centre amounted to 12 scale divisions.

This sensibility should, therefore, be ample to secure an accuracy within 1 per cent., which, for resistances of several thousand megohms, is generally sufficient.

Leakage Indicator.—The same principle of balance may be usefully adapted as a leakage indicator for electric supply circuits worked at high pressure.

For this purpose a special pattern of electrometer is requisite.

The quadrants are connected, respectively, to the two mains constituting a circuit, and the needle to earth. If the insulation of the entire circuit is good, the potential from earth of the two mains will probably be nearly equal, and the needle will remain at zero; any leakage taking place will disturb the balance to one side or the other.

By temporarily switching a small leak first on one side and then on the other, and noting the effect, the absolute value of the insulation may be approximately assessed.

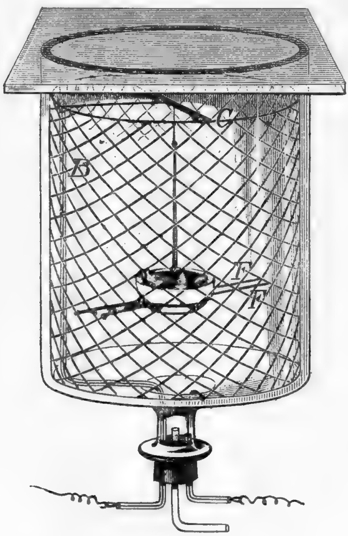
The arrangement is not applicable, however, to a concentric system of mains with alternating currents, or in any case where the capacities are large and seriously unequal.

V. "On the Electrolysis of Silver Nitrate *in Vacuo*." By ARTHUR SCHUSTER, F.R.S., and ARTHUR W. CROSSLEY, B.Sc. Received January 5, 1892.

The following investigation was undertaken in order to clear up some minor irregularities which occur when the intensity of an electric current is measured by means of a silver voltameter.

The electrolysis of silver nitrate yields with moderate precautions such very consistent results that it seemed of interest to follow up the small apparent deviations from Faraday's laws which are found to exist. One of these irregularities has been noticed by Lord Rayleigh, who found that the deposit of silver from a hot solution was about one part in two thousand heavier than the deposit from a cold solution. A second anomaly lies in a small but regular discrepancy in the deposits when these are taken simultaneously in platinum bowls of different sizes; the difference, according to our experiments, seems to depend on the current density at the *anode*. But the chief part of this paper will deal with the fact discovered by us, that the deposits are slightly larger when the electrolysis is conducted *in vacuo* than when, as usual, the voltameters are exposed to air at the ordinary pressure. This difference we trace to the effects of dissolved

FIG. 1.



oxygen, for when the electrolysis is carried out in an atmosphere of oxygen the deposits are smaller than those obtained in air.

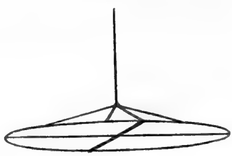
The apparatus we employed to obtain a deposit *in vacuo* is illustrated in fig. 1. An inverted bell-jar, closed at the bottom by an india-rubber stopper and at the top by a plate of glass, contains a tight-fitting cylindrical cage of wire gauze, which serves as support to the electrodes. The platinum basin is placed on two stout copper wires, F, which are soldered to the cage. Metallic contact between the wires and the bowl is secured by the help of tinfoil, which is wrapped round the wires and forms a cushion on which the bowl rests. One of the wires leading to the battery is soldered to the cage. The anode is suspended from a glass rod, C, fixed to the cage near its upper end, the current being conveyed to the anode by an insulated wire passing through a glass tube, B, which is also secured to the cage. Three pieces of glass tubing pass through the india-rubber stopper; one serves to exhaust the vessel, while the wires leading to the battery pass through the remaining two.

The stopper is rendered air-tight by means of Faraday cement, and some grease has to be used to prevent leakage between the glass plate and bell-jar. To prevent particles of this grease contaminating the solution, a tightly-fitting piece of cardboard, not shown in the figure, was placed above the cage. In the latter part of the investigation two nearly identical bell-jars were used.

The same current always passed through two or three voltameters in succession, and the deposits obtained simultaneously were compared with each other. One of the platinum bowls, to be referred to as the large bowl, has a diameter of 5 inches, while the smaller bowl had a diameter of $3\frac{1}{2}$ inches.

The silver anodes had a thickness of about 2 mm., and generally larger anodes were used in the large bowl than in the small one. With respect to the contact between the anodes and the platinum wires conveying the current, it seems worth while to draw attention to a precaution, which, if neglected, may cause serious trouble. We placed at first, for the sake of convenience, the anodes simply into two loops of platinum wire. These loops crossed at right angles as in fig. 2. The current under these circumstances is apt to pass partly from the platinum wires, and dark red crystals (probably

Fig. 2.



Ag_2O_2) then shoot out rapidly and form a bridge across the electrolyte.

We have not observed similar effects when the silver plates were perforated, and the platinum wires which passed under the silver plate were everywhere in metallic contact with it. We used filter-paper to cover the anodes, and followed generally Lord Rayleigh's instructions regarding the conduct of the experiment. The platinum basins were in some experiments first cleaned out with sand, but often this was not done. They were then washed with (1) concentrated nitric acid, (2) strong caustic soda, (3) tap water, (4) distilled water. They were dried roughly with a clean silk handkerchief and heated over a Bunsen flame. After an hour's cooling they were weighed. The deposits of silver were washed three or four times with distilled water, and allowed to stand under water for a night; they were then again washed several times and dried in an air-bath at first at 100° C.; the temperature was finally raised to 160° for ten minutes. After an hour's cooling, the final weighings were taken.

In a large number of experiments it almost certainly happens that some anomalous results are obtained, either through insufficient washing or through loss of small quantities of silver. We give, without exception, the result of each experiment, and think that on the whole they show a remarkable consistency in the indications of the silver voltameters. The effects we investigate are the differences in the deposit of less than one part in a thousand, and the possibility of investigating these differences is a proof that the electrolysis of silver nitrate can safely be trusted to that degree of accuracy.

The only serious source of error against which we had to guard was the prevention of leakage in the leads between the two voltameters.

That our results can in no way be attributed to such leakage is shown by the fact that the voltameters were used in the same position with the bell-jar exhausted or full of air. When the jar was full of air, the difference in the deposit disappeared, except for the small anomaly due to the different sizes of the basins.

When the jar is exhausted, it might be thought that a film of moisture could condense outside the platinum bowl, owing to the cooling due to evaporation in an atmosphere saturated with vapour. If such a film were to a certain extent to short-circuit the bowl, a smaller deposit would be formed *in vacuo;* but our effect is an increase, not a diminution, of the deposit. Our leads were all carefully insulated, and as the resistance of the voltameters was never more than 1 ohm, there is no difficulty in avoiding leakage to the extent required.

For the sake of clearness, we do not give our results in the order in which they were obtained, but the numbers attached to each

Table I.

Number of experiment.	Date.	Strength of solution in per cent.	Approximate current in ampères.	Duration of electrolysis in minutes.	Weight of deposits.		Difference in mgrms.	Percentage difference.
					Large bowl.	Small bowl.		
4	Jan. 15	12	0·58	34	1·3223	1·3220	+0·3	0·023
7	,, 27	12	0·70	30	1·4229	1·4226	+0·3	0·021
9	Feb. 2	12	0·59	120	4·7616	4·7606	+1·0	0·021
15	March 25	15	0·56	60	2·2364	2·2361	+0·3	0·014
16	,, 31	15	0·55	60	2·2068	2·2060	+0·8	0·036
12	March 17	12	0·78	60	3·1653	3·1686	−3·3	−0·106
13	,, 19	15	0·83	60	3·3518	3·3517	+0·1	0·003
14	,, 23	15	0·55	60	2·2410	2·2410	+0·0	0·000

experiment represent the order in which they were made. We begin by comparing together the deposits obtained in bowls of different sizes, both being in air.

With the exception of the last three observations, the results give a consistent difference of about two parts in ten thousand in favour of the larger bowl. With respect to the last three observations, we have to offer the following explanation :—In order to trace, if possible, the difference between the results obtained with large and small bowls, we used in these experiments two anodes of the same size, while in all other cases the anodes were approximately proportional to the size of the bowl. Experiment 12 is anomalous; we cannot account for the difference of 3 milligrams in favour of the small bowl, and simply record the observation; but do not think that this one experiment can render the results of the others doubtful, especially when taken in conjunction with Lord Rayleigh's observations, presently to be referred to. Experiments 13 and 14 seem to show that when anodes of the same size are used the discrepancy between the bowls disappears. This confirms an impression we have gained that the effect is possibly due to secondary products formed at the anode when the current density there exceeds a certain value. It seems certain that too great a current density at the anode is accompanied by a smaller deposit, but our experiments are not sufficient to decide whether the systematic difference in the two bowls is to be ascribed to the same cause.

Table II.—Comparison of Deposits obtained by Lord Rayleigh in Large and Small Bowls.

Date.	Deposit in large bowls.	Deposit in small bowls.	Difference in mgrms.	Percentage difference.
	grms.	grms.		
Nov. 29	3·0166	3·0165	+0·1	+0·003
Dec. 4	2·9907	2·9902	+0·5	+0·017
Feb. 18	2·3484	2·3482	+0·2	+0·009
		2·3483	+0·1	+0·004
Feb. 22	3·2977	3·2966	+1·1	+0·033
		3·2979	−0·2	−0·006
Feb. 29	2·2698	2·2693	+0·5	+0·022
		2·2701	−0·3	−0·013
Mar. 5	1·2247	1·2247	±0·0	±0·000
		1·2248	−0·1	−0·001
Mar. 10	1·0648	1·0643	+0·5	+0·047
		1·0645	+0·3	+0·028
Mar 14	1·2897	1·2892	+0·5	+0·039
		1·2893	+0·4	+0·031
			Mean..........	+0·015

Lord Rayleigh in his experiments on the silver voltameter used two bowls of approximately the same size as ours, and the foregoing comparison will show that the difference in the deposits pointed out by us also appears in his results.

In Table II we have entered in two separate columns the deposits obtained by Lord Rayleigh simultaneously from silver nitrate solutions in large and small bowls respectively.

The mean deposit in the large bowls is therefore greater by approximately the same amount as in our experiments. In three cases only were the deposits in the small bowls heavier; and in two out of these three cases the bowl showing these larger deposits contained a 30 per cent. solution, while the other at the same time was filled with a 15 per cent. solution. It seems possible, therefore, that when the strength of the solution is increased to 30 per cent. the difference due to the size of the bowls will disappear. We have recalculated Lord Rayleigh's value for the equivalent of silver, taking the deposits in the large and small bowls separately, using the weight of silver deposited before heating to verge of redness; we find for the equivalent of silver:

(*a*)	calculated from deposits in large bowls	0·0111817
(*b*)	,, ,, small ,, ...	0·0111797
	Mean	0·0111807

The heating to redness seems to affect the deposits equally, and reduces the weight, on the average, by about one part in ten thousand, which accounts for the difference between the above mean and the equivalent as given by Lord Rayleigh and Mrs. Sidgwick.

In some of our later experiments we used three voltameters in series, two of them being kept in an exhausted receiver.

This arrangement allowed us to judge whether the difference in the results obtained with large and small bowls persisted *in vacuo.* The results are not very concordant, but the average deposits are heavier in the large bowl, and hence we do not believe that the influence of current density can be ascribed to the presence of air in the solution.

In Experiment 27 the manipulation differed, in so far as the bowls were cleared out with sand before use: a proceeding adopted in the first experiments as far as the eleventh, but abandoned afterwards. We cannot, of course, draw any conclusions from a single experiment, but it does not seem impossible that the complete removal of the old surface by washing with sand renders the effect of current density more prominent. There is, no doubt, a difference in the condition under which the electrolysis is carried out, according as the deposit takes place on platinum as in the first few seconds, or on silver as in

Table III.—Pressure about $1\frac{1}{8}$ inches.

Number of experiment.	Date.	Strength of solution in per cent.	Approximate current in ampères.	Duration of electrolysis in minutes.	Weight of deposits.		Difference in mgrms.	Percentage difference.
					Large bowl.	Small bowl.		
18	April 30	20	0·5	60	1·9740	1·9736	+0·4	+0·022
19	May 4	20	0·5	60	2·0239	2·0231	+0·8	+0·040
20	„ 6	20	0·5	60	2·0088	2·0090	−0·2	−0·010
21	„ 8	12	0·5	60	2·0566	2·0562	+0·4	+0·020
22	„ 11	12	0·45	60	1·8785	1·8781	+0·4	+0·021
23	„ 13	12	0·75	40	2·0470	2·0469	+0·1	+0·005
24	„ 20	12	0·75	40	2·0255	2·0252	+0·3	+0·015
25	„ 25	12	0·5	30	1·0117	1·0120	−0·3	−0·030
26	„ 27	12	0·5	30	0·9795	0·9796	−0·1	−0·010
27	„ 29	12	0·5	30	1·0057	1·0039	+1·8	+0·180
		Sum including 27			17·0112	17·0076	+3·6	+0·021
		Sum excluding 27			16·0055	16·0037	+1·8	+0·011

the later stages, and after a number of experiments there may be a thin layer of silver, possibly an alloy of silver and platinum, which resists the action of acid, and can only be scraped out with sand. It is to be noted that Kohlrausch took his silver deposits on platinum which had previously been covered with a layer of silver; while in Lord Rayleigh's experiments the silver deposits were removed from the dish before a new experiment was made. The difference may account for the somewhat greater equivalent found by Kohlrausch; but the concordance of the results shows that there can be no systematic difference amounting to more than a few parts in ten thousand.

We turn now to the main part of the investigation, which is the comparison of the deposits obtained in air and *vacuo.* The solution used in the different voltameters was always taken out of the same bottle. We had intended in this way to make sure that any difference in the deposits was not due to some chemical difference in the solutions. It did not occur to us at the time that the solution in one voltameter being freed of air, we should gradually diminish the amount of air also in the other voltameter, for the solutions were kept in stoppered bottles, which did not allow of a ready re-absorption of oxygen. It will be seen that the differences in the deposits, when these were taken in air and *vacuo*, were larger and more regular in the first experiments than later on, and this may have been due to the gradual elimination of oxygen out of the solution.

Our first experiments were made with the large bowl placed *in vacuo*, and the small one in air. The results, to which a later one is added for the sake of completeness, are embodied in Table IV. Experiment 17 was not a satisfactory one, as will be explained later on, and is therefore included in square brackets.

The large difference between the result obtained in air and *in vacuo* first drew our attention to a possible influence of the size of the bowl. The experiments made to clear up this point have already been described. A few deposits were taken with the small bowl *in vacuo* and the large bowl in air; although the two effects counteract each other, the deposits *in vacuo* are larger than those in air, as is shown by Table V.

On the supposition that the effect due to the size of the bowl is the same in air as it is *in vacuo*, we may combine the results of Tables IV and V, and thus find that the deposits of silver *in vacuo* are about one part in a thousand larger than those in air. The next two experiments (Table VI) were a surprise.

Table IV.—Comparison of Deposits in Air at Atmospheric Pressure and under a Reduced Pressure of about $1\frac{1}{2}$ inches.

Number of experiment.	Date.	Strength of solution in per cent.	Approximate current in ampères.	Duration of electrolysis in minutes.	Weight of deposits.		Difference in mgrms.	Percentage difference.
					Large bowl *in vacuo.*	Small bowl in air.		
1	Jan. 9	12	0·6	31	1·2468	1·2451	1·7	0·141
3	,, 14	12	0·6	30	1·2005	1·1989	1·6	0·133
17	April 27	20	0·5	60	[2·0183]	[2·0144]	[3·9]	[0·195]

Table V.—Comparison of Deposits in Air at Atmospheric Pressure and under a Reduced Pressure of about $1\frac{1}{2}$ inches.

Number of experiment.	Date.	Strength of solution in per cent.	Approximate current in ampères.	Duration of electrolysis in minutes.	Weight of deposits.		Difference in mgrms.	Percentage difference.
					Small bowl *in vacuo.*	Large bowl in air.		
5	Jan. 21	12	0·7	38	1·7255	1·7243	1·2	0·070
6	,, 23	12	0·6	33	1·4361	1·4350	1·1	0·078
8	,, 29	12	0·6	120	5·1036	5·1014	2·2	0·043

Table VI.—Comparison of Deposits in Air at Atmospheric Presssure and under a Reduced Pressure of about $1\frac{1}{2}$ inches, the Anode *in vacuo* being small.

Number of experiment.	Date.	Strength of solution in per cent.	Approximate current in ampères.	Duration of electrolysis in minutes.	Weight of deposits.		Difference in mgrms.	Percentage difference.
					Small bowl *in vacuo*.	Large bowl in air.		
10	Feb. 5	12	0·6	120	4·8455	4·8477	−2·2	−0·046
11	„ 7	12	0·9	86	5·1829	5·1830	−0·1	−0·000

We traced the cause of the anomalous results shown in this table. The anode of the small bowl had by repeated use been gradually dissolved; the current density was consequently increased. Under these circumstances the current becomes unsteady, polarisation effects make themselves apparent, and the deposits are no longer trustworthy. The deposits taken when the current density is too great have generally a yellow colour. We are reminded of some old experiments in which by increasing the current density black deposits were obtained on the kathode, which at one time were supposed to be a hydride of silver. Poggendorff is generally stated to have proved that the black deposit is not a compound, but silver in a finely divided state. On referring to Poggendorff's paper, his experiments do not seem convincing, and he has expressed himself with more caution than those who quoted him afterwards. He states, however, that the black deposit often suddenly changes into a light one. Some observations made by Mr. Hoskins Abrahall in the Owens College Laboratory, as well as our own experiments, lead us to believe that it is the current density at the anode more than that at the kathode, which introduces the anomalous results. When the deposits are thus untrustworthy, the current, as far as we are able to judge, is always unsteady, so that no danger arises when the silver voltameter is used for the calibration of instruments.

At this stage of the inquiry we introduced a second bell-jar and a second voltameter of approximately the same size as the small one previously used. The balance was also changed, and the weighings were taken on a new short-beam balance. This balance was unsteady in its indications after first setting up, and a sudden change of zero while one of the basins was being weighed renders the result of Experiment 17 doubtful. The numbers obtained in this experiment are therefore included in square brackets in our tables. Table VII gives the comparison of the deposits in air and *in vacuo* taken in basins of nearly the same size. A glance at the numbers can leave no doubt as to the reality of the increase in the deposit under reduced pressure, although the amount of the increase is a little uncertain. There is only one case (Experiment 23) in which the deposits are practically identical, and in that case it was noticed that the deposit *in vacuo* was yellow—an indication that the current density was probably just a little too large. The average difference between the deposits is about one part in two or three thousand.

Table VII.—Comparison of Deposits in Bowls of the same size, taken under Atmospheric Pressure and a Reduced Pressure.

Number of experiment.	Date.	Strength of solution in per cent.	Approximate current in ampères.	Duration of electrolysis in minutes.	Pressure in bell-jar in inches.	Weight of deposits.		Difference in mgrms.	Percentage difference.
						In vacuo.	In air.		
17	April 27	20	0·5	60	2	[2·0135] or [2·0150]	[2·0144]	?	?
18	,, 30	20	0·5	60	$1\frac{1}{4}$	1·9736	1·9726	1·0	0·052
19	May 4	20	0·5	60	$1\frac{3}{16}$	2·0231	2·0228	0·3	0·015
20	,, 6	20	0·5	60	$1\frac{1}{4}$	2·0090	2·0085	0·5	0·025
21	,, 8	12	0·5	60	$1\frac{1}{4}$	2·0562	2·0559	0·3	0·015
22	,, 11	12	0·45	60	$1\frac{1}{8}$	1·8781	1·8778	0·3	0·016
23	,, 13	12	0·75	40	$1\frac{1}{8}$	2·0469	2·0470	−0·1	−0·005
24	,, 20	12	0·75	40	$1\frac{1}{8}$	2·0252	2·0248	0·4	0·020
25	,, 25	12	0·5	30	$1\frac{1}{8}$	1·0120	1·0116	0·4	0·040
26	,, 27	12	0·5	30	$1\frac{1}{8}$	0·9796	0·9787	0·9	0·090
27	,, 29	12	0·5	30	$1\frac{1}{8}$	1·0039	1·0037	0·2	0·020
29	Aug. 17	12	0·5	60	$1\frac{1}{8}$	1·8523	1·8495	2·8	0·152
30	,, 19	12	0·5	60	$1\frac{1}{8}$	1·9003	1·8990	1·3	0·070
31	,, 24	12	0·5	60	$1\frac{1}{8}$	1·8999	1·8989	1·0	0·055
								Mean.........	0·043

In the last three experiments, which gave comparatively large differences, the solutions used were kept separate between the experiments, and this leads us to think that we had previously committed an error in mixing our solutions, which, as has already been stated, must gradually have become free of air. Experiment 29 shows, however, too great a difference; some of the silver in the bowl kept in air may have been lost in the washing.

It seems remarkable that the electro-chemical equivalent of silver as deduced from the electrolysis *in vacuo* is almost identical with that obtained in Lord Rayleigh and Mrs. Sidgwick's deposits from hot solutions.

One point as yet remains to be discussed. It was reasonable to assume that the increased deposit *in vacuo* was due to the removal of the oxygen out of the solution. In order to obtain more definite information, we took some deposits in an atmosphere of oxygen. In the first experiment the two bell-jars were exhausted, and one of them filled with oxygen, which was allowed to stand for three hours over the solution before electrolysis.

The result was as follows:—

Deposit in air	(small bowl)	....	1·8618
,, oxygen	,,	....	1·8618
,, ,,	(large bowl)	....	1·8624

There is here no difference except that due to the size of the bowl. As it seemed doubtful whether the oxygen had in the course of three hours been absorbed to its full extent by the solution, three more experiments were made and conducted as follows—

One small basin was kept in air as before; the other was kept *in vacuo*, while the large basin was filled with a solution which after boiling had a stream of oxygen passed through until it was considered that the liquid was saturated with the gas. The solution thus prepared was kept in an atmosphere of oxygen. The comparison between the deposits in air and *vacuo* have already been given (Experiments 29, 30, 31, Table VII).

The weight of the deposits in air and in oxygen was as follows:—

Small bowl in air.	Large bowl in oxygen.	Percentage difference.
1·8495	1·8488	0·04
1·8990	1·8983	0·04
1·8989	1·8981	0·04

We attribute the consistency of these results partly, at any rate, to the fact that the solutions used in the three bowls were kept separate

In looking at the figures it must be remembered that the large bowl would, if placed in air, have given a larger deposit than the small one, so that the difference between oxygen and air is really greater than would appear from the numbers. There seems little doubt, therefore, that it is the removal of oxygen which is the cause of the increased deposits *in vacuo.*

We have made a number of experiments on the polarisation of the electrodes in our silver voltameters. It does not follow that because there is as much silver dissolved as deposited, there is necessarily complete absence of what is commonly called polarisation. In the first place, the silver is dissolved from a compact sheet which is in a molecular condition different to the crystalline form in which it is deposited. Secondly, the silver is dissolved into a more concentrated solution than that out of which it is deposited, and, as Warburg has pointed out, it is very difficult to distinguish polarisation effects from electromotive forces due to differences of concentration. Our experiments have shown a small but very consistent polarisation of 0·007 volt, which was the same *in vacuo* and in air.

If, after the polarising current has passed, the anode is taken out, and replaced after the liquid has been thoroughly stirred, the polarisation is reduced, but still exists to the extent of about one-third the original value. The electromotive force of polarisation does not seem to be different when the platinum basin is partially or completely covered with silver; but the greater the amount of silver the more slowly does the polarisation die out. We cannot draw any very definite conclusions from these observations, but it seemed worth while to put them on record.

We do not wish to enter into a full discussion of the explanation of our results, but only draw attention to two phenomena investigated by Helmholtz and Warburg respectively. It was shown by Helmholtz that the small current which passes through water under the action of electromotive forces insufficient to decompose it is due to the presence of dissolved oxygen. If part of the current in a solution of silver nitrate is conveyed by hydrogen atoms, no hydrogen could separate out as gas, but a recombination with the dissolved oxygen could take place. A small fraction of the current might be conveyed precisely in the way described by Helmholtz. In a subsequent paper,* Helmholtz draws from thermodynamic principles the conclusion that "in very dilute solutions or in acids containing no salts at all, metals, which we otherwise consider unoxidisable in the acid, may dissolve to a small extent with evolution of hydrogen."

Warburg,† in an important paper, shows that voltaic cells may be formed by two pieces of the same metal, dipped into the same solu-

* 'Collected Works,' vol. 2, p. 978.

† 'Wiedemann, Annalen,' vol. 38, p. 321.

tion, if the solution surrounding one of the electrodes contains oxygen in solution. He establishes, further, the fact that in such cases the metal actually enters into solution, and explains thereby a variety of phenomena. From his observations there seems little doubt that even in a solution of silver nitrate silver may dissolve to a slight extent. The amount so dissolved is possibly increased when the silver is in the nascent state, and may then become measurable.*

We draw the general conclusion from our experiments that the true electrochemical equivalent of silver is probably not quite one part in a thousand greater than the value given by Lord Rayleigh, but that, if the experiments are conducted in air and under circumstances similar to those under which Lord Rayleigh's measurements were made, the anomalies described by us do not interfere with the use of the silver voltameter as a current measurer. On the contrary, the fact that we were able to show the existence of systematic differences amounting to not more than two parts in ten thousand is a proof of its trustworthiness.

VI. "A new Mode of Respiration in the Myriapoda." By F. G. SINCLAIR (formerly F. G. HEATHCOTE), M.A., Fellow of the Cambridge Philosophical Society. Communicated by A. SEDGWICK, F.R.S. Received January 8, 1892.

[This is a new version of the Paper read November 26, 1891. See *ante*, p. 200.]

VII. "The 'Ginger-beer Plant,' and the Organisms composing it: a Contribution to the Study of Fermentation-yeasts and Bacteria." By H. MARSHALL WARD, M.A., F.R.S., F.L.S., Professor of Botany at the Forestry School, Royal Indian Engineering College, Coopers Hill. Received January 14, 1892.

[The Paper printed at page 261, *ante*, contains the substance of this Paper in abstract.]

* I have assured myself by experiment that the well-known diminution in weight of copper in copper sulphate does not take place *in vacuo*, care being taken to remove the dissolved oxygen completely. Experiments are at present in progress to investigate the electrolysis of copper *in vacuo*.—A. S.

Presents, January 21, 1892.

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Twenty-two Volumes, various, viz., seven vols. 4to and fifteen vols. 8vo. Also fifty-five parts of 'Philosophical Transactions,' and 217 Nos. of 'Proceedings of the Royal Society.' The Relatives of the late Mr. W. H. L. Russell, F.R.S.

January 28, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Melting Points of the Gold-Aluminium Series of Alloys." By W. C. ROBERTS-AUSTEN, C.B., F.R.S. Received January 25, 1892.

The author has already described and exhibited to the Society a new alloy of gold and aluminium, $AuAl_2$, which is remarkable for its intense purple colour.

The physical constants of the gold-aluminium alloys are being determined and the results will soon be ready for publication, but the series has been found to have one interesting peculiarity which deserves special mention. The author has shown ('Roy. Soc. Proc.,' vol. 49, 1891, p. 347) that the addition of 0·2 per cent. of aluminium to gold produces an appreciable fall in the freezing point, an addition of 0·4 per cent. causing a fall of 14·28°, or an "atomic fall" of 5·0° C.

These facts indicated that it was desirable to ascertain what are the melting points of the gold-aluminium series of alloys generally, and this has now been done with the aid of the Le Chatelier thermo-couple used in the way which was previously described (*loc. cit.*).

The results show that, although a white alloy, containing 10 per cent. of aluminium, has a melting point which is no less than 417° lower than that of gold; the purple alloy, on the other hand, melts

at a point which has yet to be definitely fixed, but which is several degrees above gold.* In fact, when workmen who are accustomed to melt gold on a large scale, attempt to melt this purple substance, they find it difficult to believe that they are dealing with a gold alloy, as it is so infusible.

The melting points of the rest of the series richer in aluminium appear to fall continuously to 660°, a little below the melting point of aluminium (665° C.).

The purple alloy presents the only case, known to the author, of an alloy, free from mercury, having a higher melting point than that of the least fusible of its constituents, and he considers that this fact affords strong evidence of its being a true compound of gold and aluminium.

It is generally admitted that there are true compounds in the copper-tin series, for $SnCu_3$ and $SnCu_4$ seem to be well defined, but their melting points are much lower than that of copper.

A. P. Laurie has just shown ('Phil. Mag.,' January, 1892) that in the gold-tin series, the alloy containing 63 per cent. of gold and 37 per cent. of tin has an electromotive force which distinguishes it from the rest of the series and points conclusively to its being a true compound, but the author finds that it melts readily below redness.

The melting points of ordinary chemical compounds are often much higher than the melting point of the least fusible constituent. *Galena*, for instance, melts at a strong red heat; it is difficult to fix the point accurately as the substance volatilizes, but it is close to 900° C. Its constituents, lead and sulphur, melt at 335° and 115° respectively. *Stibnite* also, sulphide of antimony, melts at about 530°, according to Dr. Joly, while antimony fuses at 440°.

The gold-aluminium series is of unusual interest, and well deserves careful attention.

* [Two very careful experiments were made, each with 40 grammes of the alloy, the cooling curve being traced by the autographic recorder already described ('Roy. Soc. Proc.,' *loc. cit.*). These curves gave 1065° and 1070° respectively as the melting point of the alloy $AuAl_2$, the mean of which is 32·5° higher than the melting point of gold. If, however, small quantities of the alloy be fused before the oxy-hydrogen blowpipe, it is easy to obtain a lower result, as aluminium is readily burnt out from the little mass. The composition of the alloy is thereby changed to one of the series richer in gold, of which the melting points are lower than that of gold.—Feb. 9, 1892.]

II. "Colour Photometry. Part III." By Captain W. de W. Abney, C.B., R.E., D.C.L., F.R.S., and Major-General Festing, R.E., F.R.S. Received December 14, 1891.

(Abstract.)

The authors refer to their paper on Colour-Photometry (Bakerian Lecture, 1886), in which a method was given of forming a curve of luminosity of the spectrum, the source of light being the crater of the positive pole of an electric arc lamp.

They point out that in making the observations for forming this curve no attention was paid to the part of the retina of the eye which was used, and which embraced the "yellow spot" and some of the surrounding portion.

In their further researches this point came to be of importance, and they describe how, by modifications of the apparatus and of the methods of observing, they were able to use either the yellow spot or portions outside it, and they give the results of the observations, showing how the curves become modified in each case.

The absorption by the yellow spot takes place in all rays more refrangible than E; but to the less refrangible rays the outer part of the retina is less sensitive than the central part.

The Limit of Colour Vision.—It is well known that when light of any colour becomes enfeebled to a certain degree, the eye fails to see colour, though it may still recognise the existence of light. Observations were made to determine the point at which, for each part of the spectrum, the sensation of colour is lost. The same apparatus as before was used for forming the spectrum and the "reference" beam of white light, and a supplementary apparatus was devised for reducing the beam of light and for measuring the amount of reduction. Each coloured beam was reduced until, in comparison with a feeble white beam, it appeared colourless. The amount of reduction in each case being measured, a curve was plotted showing the proportional reduction in part of the spectrum. The absolute intensity of the beam from D having been measured by comparison with an amyl acetate lamp, that of each other part of the spectrum was calculated by aid of the luminosity curve above referred to. It then became possible to plot a curve which shows the intensity of the original source at which, in each part of the spectrum, colour first becomes visible.

The portion of the spectrum in which colour is visible from the feeblest source is between about λ 500 and λ 615. This accounts for the fact that in a feeble light, such as that of the moon, objects appear to be of a greenish hue, and also that moonlight passing through coloured windows does not give a coloured image in most cases.

Extinction of the Light of different Parts of the Spectrum.—A full description is given of the apparatus used and of the method of observation for determining how much the light of each part of the spectrum must be reduced in order that it may be extinguished. Observations were made (1) with the central part of the eye only and (2) with the whole eye.

From these observations curves were plotted, showing the proportion of the beam from each part of the spectrum which was just not visible. These are called extinction curves. They differ only in that part of the spectrum where the yellow spot absorption takes place. The minimum ordinate is at about λ 5300, and represents $65/10^7$, that being the proportion to which the beam had been reduced at extinction, the intensity of the unreduced beam from D in the same spectrum being that of an amyl acetate lamp at 6 feet. The intensity of other beams of the spectrum was calculated from this by the aid of the luminosity curve as before. A curve was then derived from each of the extinction curves by taking as ordinates the product of such ordinate of an extinction curve and the luminosity of the corresponding beam; these derived curves then represent (on the supposition that all the beams were originally of the same luminosity as D), the proportion, and therefore the absolute intensity, of any beam which would be just not visible. These two curves differ slightly at the part affected by the yellow spot, but that for the whole eye is horizontal from the extreme violet end to about λ 4800; it then rises rapidly to λ 6840, and again becomes horizontal. This seems to confirm the view that a single sensation only is excited by each of the ends of the spectrum.

The reciprocals of the ordinates of either of the first two extinction curves being taken, what is called a "persistency" curve is formed. The curve for the whole eye and that for the central portion are given. It is reasonable to expect that the "persistency" curve should have relation to some colour sensation of the eye, which may perhaps be looked upon as the dominant sensation, as it is excited by the smallest quantities of light.

An examination with the results of observations made by colour-blind people is then entered on.

A gentleman, M., made a series of observations. He has two colour sensations only, which he calls "red" and "black." Yellow he describes as "white," green as "bright black," blue as "darker black." His luminosity curve has been plotted on such a scale that the red portion corresponds with that of the normal curve. The rest, however, falls below this, and it leaves off a little beyond F. The curve formed by the differences of his and the normal ordinates may be considered to be M.'s deficiency curve.

Two brothers, P. and Q., were also examined. They have the same

vision, which is monochromatic. Their luminosity curve, M.'s deficiency curve, and the normal persistency curve correspond very nearly except in the part affected by the yellow spot. It therefore appears as if P. and Q. had only the sensation which is looked upon as the dominant sensation in the normal eye, and of which M.'s eye is devoid, and that P.'s and M.'s eyes together would make up a normal eye.

The results are also given of the examination of the vision of a red-blind (H. R.) and of a green-blind person (V. H.). Their "persistency" curves, as well as that of P., nearly correspond generally with each other and with that of normal vision. The "absolute intensity" extinction curves of H. R. and V. H. also do not differ notably from the normal; but in P.'s case the ordinates are larger, from which it may be inferred that his sense of vision is less acute.

Assuming blue, green, and red to be the three primary sensations, P.'s and Q.'s luminosity curve or M.'s deficiency curve would represent the first, and V. H.'s deficiency curve the second, but H. R.'s deficiency curve would not quite represent the third, as he is not entirely devoid of appreciation of red.

Luminosity Curve of Spectrum of Low Intensity.—The normal persistency curve being apparently the same as the luminosity curve of persons with but one colour sensation, experiments were made to determine what would be the luminosity to the normal eye of the different parts of a spectrum of a very low intensity. A spectrum was formed of which the beam from D was equal in intensity to 1/132·5 of an amyl acetate lamp at 1 foot. The luminosity curve of this was found to correspond very nearly to that of P., and to the normal persistency curve. By reducing the light in less degrees, luminosity curves were produced corresponding to those of persons more or less red-blind.

Experiments were then made to ascertain whether this change in the relative luminosities of the different rays would continue to vary with constantly increasing intensity of the light, or whether a point would be reached after which the curve, when it had the same maximum ordinate, would be constant.

A beam being taken from one point in the spectrum, it was compared with the reference (white) beam. Rotating sectors were placed in both beams, and equality of luminosity thereby produced. The aperture of one set of sectors being varied, the alteration of the other which was necessary to re-establish the equality of illumination in each case was noted. Curves were then plotted for several rays, of which the ordinates represent the apertures in the coloured beams and the abscissæ those in the white beam when the illumination is equal. Clearly, if these curves ever became straight lines for all parts of the spectrum, the luminosity curve would become constant.

This was found to be the case. The curve of the beam from scale No. 46·3 of the spectrum (about λ 5613) was found to be straight from the origin. Those of beams of greater refrangibility were at first concave to the axis of abscissæ, those of less refrangibility convex; but all had become straight before an intensity of 1/60 of an amyl acetate lamp at 1 foot had been reached.

III. "On certain Ternary Alloys. Part V. Determination of various Critical Curves, and their Tie-lines and Limiting Points." By C. R. ALDER WRIGHT, D.Sc., F.R.S., Lecturer on Chemistry and Physics in St. Mary's Hospital Medical School. Received November 19, 1891.

The triangular method of graphical representation suggested by Sir G. G. Stokes, and described in Part IV ('Roy. Soc. Proc.,' vol. 49, p. 174), substantially amounts to the tracing out of a curve ("critical curve") which shall express the saturation of the solvent C with a mixture in given variable proportions of the other two constituents, A, B; the variation being such that any given point on the curve is related to some other point ("conjugate point") in a way given by the consideration that all mixtures of the three constituents, A, B, C, represented by points lying on the line ("tie-line") joining these two conjugate points ("ideal" alloys, or mixtures), will separate into two different ternary mixtures corresponding with the two points respectively; whereas any mixture of the same constituents, represented by a point lying *outside* the critical curve, will form a "real" alloy, or mixture, not separating spontaneously into two different fluids but existing as a stable homogeneous whole.

The experiments described in Part IV unmistakably point to the conclusion that *whenever sufficiently intimate and prolonged intermixture of the three constituents can be effected, there is no variation whatever* in the position of the point experimentally determined as conjugate to some other given point on the curve, no matter what may be the proportions subsisting between the three constituents employed; but that when metals are used, the practical difficulty in effecting thorough intermixture by stirring when molten is occasionally so great as to lead to slight, but sensible, differences in the composition of the ternary alloys formed simultaneously with some one given alloy approximately conjugate thereto, in different cases where the relative proportions of the constituents are materially different.

A large number of additional experiments on this point have been made, the general result of which is completely to corroborate and

confirm these conclusions; when the proportions of the constituents are such that approximately equal quantities of the two different ternary alloys are formed, the error due to incomplete intermixture is generally a minimum; this obviously happens when the constituents are used in such proportions as to represent an "ideal" alloy corresponding approximately with the central point of the tie-line uniting the two conjugate points. When, however, the proportions of the constituents differ materially from these, representing an "ideal" alloy considerably nearer to one conjugate point than to the other, so that one of the two ternary alloys is formed in much larger quantity than the other, the effect of incomplete intermixture becomes more marked, more especially when the *heavier* of the two alloys largely preponderates.

In cases where the critical curve is approximately symmetrical with respect to the central line of the triangle drawn from its apex perpendicular to the base, so that the "limiting point" lies near to the apex, the minimum error due to incomplete intermixture is accordingly observed when the two immiscible metals, A, B, are used in approximately equal quantities throughout. If, on the other hand, the critical curve is unsymmetrical, in order to minimise the error due to imperfect intermixture (more especially as regards those parts of the curve where the proportion of the "solvent," C, is greatest), the other two metals, A, B must *not* be used in equal quantity, but the one or the other must be in excess according as the "limiting point" lies on the right or the left-hand side of the central line. Thus, as shown below, in the case of mixtures of chloroform, acetic acid, and water, the limiting point lies considerably to the *left* (chloroform side) of the central line; similarly with mixtures of lead, silver, and zinc, the limiting point is also considerably to the *left* (lead side) of the central line. In such cases the heavier of the two immiscible constituents, A, B (chloroform, lead) must be made to predominate considerably over the lighter one (water, zinc), in order that approximately equal quantities of the two ternary mixtures may be formed. On the other hand, with mixtures of lead (or bismuth), tin, and zinc, the limiting point lies sensibly to the *right* of the central line (zinc side); so that to determine as accurately as possible the positions of the conjugate points situated towards the limiting point, zinc must be made to predominate over lead (or bismuth) in the mixtures employed.

The effect of this in practice is that in order to trace out the critical curve for a given trio of metals the most expeditious way is to begin by preparing two or three mixtures with small quantities of solvent C and approximately equal quantities of A and B; from the results obtained with these mixtures, represented graphically on Stokes' triangular system, a fair idea can generally be formed as to

what must be the proportion between A and B, in order to yield with a somewhat larger quantity of C an "ideal" alloy that will separate into approximately equal quantities of the two ternary alloys formed therefrom. Similarly, from these further results, the proportions requisite for the accurate determination of other pairs of conjugate points higher up still can be inferred; and so on. In this way the critical curve is gradually traced out with a much smaller proportion of wasted labour (owing to imperfect formation of truly conjugate ternary alloys) than would otherwise be the case. This method of procedure has accordingly been adopted in the investigation of various critical curves of the kind which will be discussed in a future paper.

These considerations obviously suggest the possibility that, in certain cases at any rate, the mean curve values deduced in the earlier parts of this research may require some little degree of revision, inasmuch as some of the pairs of alloys simultaneously formed were derived from mixtures yielding one alloy in much larger quantity than the other; so that some small amount of experimental error, due to imperfect formation of truly conjugate alloys, might exist. Accordingly, a considerable number of the experiments that might possibly be faulty from this cause have been repeated, using proportions of A and B better suited to the end in view, viz., formation of the two ternary alloys produced in not widely different quantities relatively to one another. Further, a variety of additional experiments have been made with the object of deducing the situations of pairs of conjugate points lying nearer to the limiting points than those previously determined. These further experiments, however, have demonstrated the existence of two other sources of error, not noticeable to so great an extent at the other parts of the critical curve.

In the first place, when an "ideal" alloy is used corresponding with a pair of conjugate points lying near to the limiting point, the two alloys formed differ far less widely in composition than is the case with other pairs more removed from the limiting point; consequently, the densities of the two alloys do not greatly differ, which circumstance appears greatly to impede their complete separation from one another by gravitation whilst standing at rest molten. Secondly, such "ideal" alloys appear to be extremely sensitive to slight differences of temperature, at least as compared with mixtures not so near the limiting point; so that whilst a difference of 10°, 20°, or even 50° C. makes but little difference in the compositions of the two ternary alloys formed from a given "ideal" alloy not near the limiting point, it produces a marked effect on an "ideal" alloy near to that point; in some cases, indeed, a rise of a few degrees in temperature will suffice to transform an "ideal" alloy separating into

two ternary alloys considerably different from one another into a "real" alloy not separating at all, but remaining perfectly homogeneous at this slightly higher temperature. Leaving out of sight the practical impossibility of maintaining a lead-bath at 600°—800° C. at an absolutely constant temperature (within, say, $\pm 10°$) for eight or ten hours together, the lowering of the temperature taking place on removing the clay test-tube from the lead-bath sometimes appears to cause a measurable amount of separation of a different heavier alloy from the lighter one formed in the bath, and *vice versâ*, during the short time that elapses before the compound mass becomes solid. So that, in fine, duplicate experiments with "ideal" mixtures situated near to the limiting point are apt to yield discordant results owing to one or other of these causes, or to the two combined; and in consequence the direct determination of the exact position of the limiting point is impracticable, although of course its situation can be approximately deduced from the graphical representation of the results on the triangular system. Still nearer approximations can be obtained by the use of other methods kindly suggested by Sir G. G. Stokes, and more fully described below.

The present paper deals with the results of the further experiments made, as above stated, with the mixtures partly described in the previous four Parts* (lead-tin-zinc, bismuth-tin-zinc, lead-silver-zinc, bismuth-silver-zinc, chloroform-acetic acid-water), for the purpose of more completely determining the exact positions of the critical curves for certain definite temperatures, and the systems of tie-lines and their limiting points pertaining to each curve respectively. In a subsequent paper the analogous curves will be described, derived from the combinations of lead or bismuth with zinc as A, B, and cadmium or antimony as C, and with various analogous ternary mixtures where aluminium takes the place of zinc.

Mixtures of Chloroform, Water, and Acetic Acid.

The experiments described in Part IV were continued, using mixtures containing more chloroform than water (from 2 to 3 parts chloroform to 1 of water), and nearly, but not quite, enough acetic acid to form a homogeneous fluid, the composition required being arrived at by adding enough acetic acid to form a single fluid, and then dropping in a little water or chloroform, or both, until separation ensued. With mixtures represented by points lying close to the limiting point, it was found that comparatively slight variations in temperature produced marked alterations in the composition of the two fluids into which the mixture separated, more especially as

* Part I, 'Roy. Soc. Proc.,' vol. 45, p. 461; Part II, vol. 48, p. 25; Part III, vol. 49, p. 156; Part IV, vol. 49, p. 174.

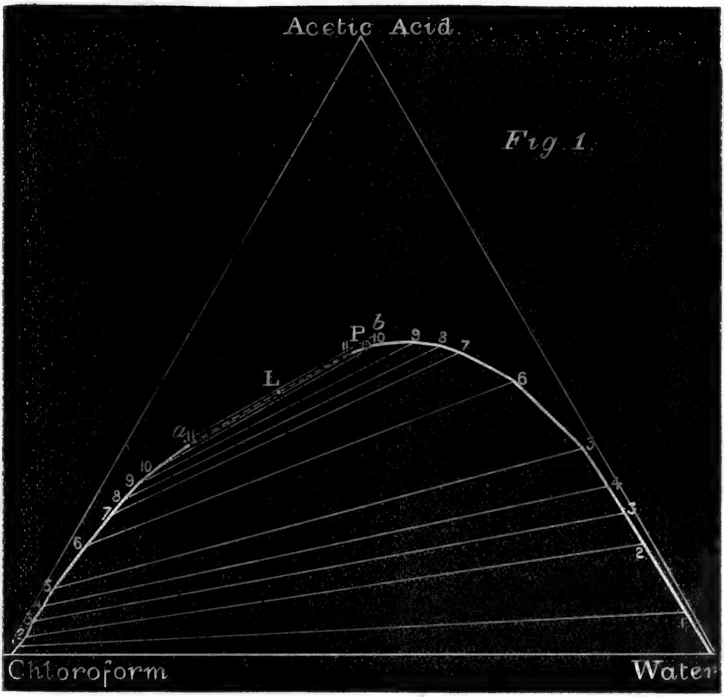
regards the lighter fluid; thus the same mixture yielded the following figures, according as the temperature at which the mass was maintained during the period of agitation and standing lay near to 20°, 16°, or −4° respectively; whilst at temperatures above 25° a single homogeneous fluid was formed, not separating at all until the temperature was slightly lowered.

Temperature.	Heavier fluid.			Lighter fluid.		
	Chloroform.	Water.	Acetic acid.	Chloroform.	Water.	Acetic acid.
20°	55·62	9·60	34·78	30·23	21·89	47·88
16°	56·19	9·56	34·25	26·10	26·04	47·86
−4°	58·88	7·35	33·77	23·00	25·75	51·23

In order to obtain numbers as nearly as possible comparable with one another and with those previously described in Part IV (all of which were obtained at temperatures pretty close to 18°), the mixtures employed were examined at temperatures somewhat above, and also at a little below 18°, in such fashion that the average numbers obtained with each mixture should represent the mean compositions for an average temperature sensibly = 18°. The following average numbers were thus obtained in three such cases:—

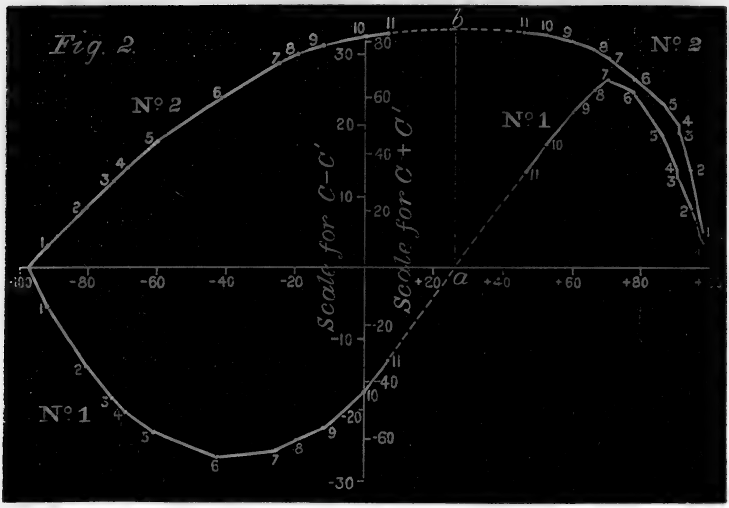
No. of tie-line.	Heavier fluid.			Lighter fluid.		
	Chloroform.	Water.	Acetic acid.	Chloroform.	Water.	Acetic acid.
9	67·15	5·20	27·65	18·33	31·11	50·56
10	59·99	7·93	32·08	25·20	25·39	49·41
11	55·81	9·58	34·61	28·85	23·28	47·87

Fig. 1 represents these values, together with those previously described in Part IV, the tie-lines numbered 1 to 8 being those previously described, and those marked 9, 10, and 11, the above three sets of average values respectively. The line *ab* is the tie-line uniting the two points obtained as above-mentioned with one mixture examined at −4° C., obviously belonging to a critical curve lying *outside* the curve for 18°. The point P indicates a mixture of chloro-



form 25 per cent., water 25 per cent., and acetic acid 50 per cent., not separating into two fluids at 18°, as previously described. The point marked L is the "limiting point," deduced from the above figures by means of two different graphical methods suggested by Sir G. G. Stokes.

By the first method, the percentages of chloroform, water, and acetic acid in the heavier liquid being respectively indicated by A, B, and C, and those in the lighter liquid by A′, B′, and C′, the values of A−B (positive quantities) are plotted off as abscissæ to the right, and those of A′−B′ (negative quantities) to the left of the origin, the values of C′−C (positive) being used as ordinates for the first set of abscissæ, and those of C−C′ (negative) for the second set. Fig. 2, curve No. 1, represents the plotting thus obtained; by joining the ends of the two portions of curve thus laid down by the dotted line 11, 11, as shown, a point, *a*, is deduced where this dotted line cuts the base line. This point, *a*, corresponds with the "limiting point," where the lighter and heavier alloys merge into one; when the plotting is carefully made on a sufficiently large scale, the position of



a is found to correspond with the value A−B = 25·9. In similar fashion the values of C+C′ are plotted off as ordinates to each of the two sets of abscissæ, and the ends of the two portions of curve No. 2 thus obtained also joined, as shown by this dotted line. A point, b, is thus deduced where this dotted line cuts the perpendicular to the base at the point a. The length ab thus represents the value of C+C′ for the point a, *i.e.*, at the "limiting point," when obviously C = C′; when the scale is sufficiently large this is found to correspond with the value of C+C′ = 83·5. From these two values for A−B and C+C′ (= 2C), the following values for A, B, and C result:—

$$C = C' = \frac{83{\cdot}5}{2} = 41{\cdot}75$$

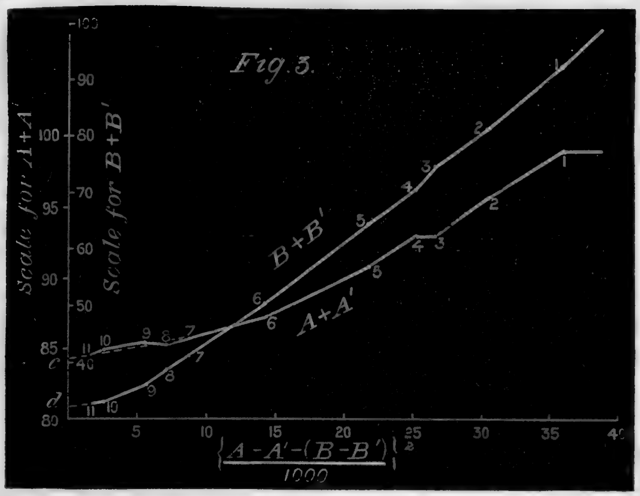
$$\text{whence } A+B = 100-41{\cdot}75 = 58{\cdot}25$$

$$A = \frac{A+B+(A-B)}{2} = \frac{58{\cdot}25+25{\cdot}9}{2} = 42{\cdot}07$$

$$B = \frac{A+B-(A-B)}{2} = \frac{58{\cdot}25-25{\cdot}9}{2} = 16{\cdot}18$$

$$\overline{100{\cdot}00}$$

By the second method. A, B, C, and A′, B′, C′, having tne same meanings as before, two curves are plotted, each with the values of $\{A-A'-(B-B')\}^2$ as abscissæ, one with the values of A+A′ as



ordinates, the other with those of B+B′. Fig. 3 represents the two curves thus obtained; by prolonging these, as shown by the dotted lines, two points are obtained, *c* and *d*, respectively representing the values of A+A′ and B+B′, when the abscissa becomes 0, which corresponds with the position of the "limiting point." With a sufficiently large-scale plotting, the points *c* and *d* are found to correspond respectively with the values 84·5 and 32·5; and as at the "limiting point" A = A′, and B = B′, it thence results that—

$$A = \frac{84{\cdot}5}{2} = 42{\cdot}25$$

$$B = \frac{32{\cdot}5}{2} = 16{\cdot}25$$

$$C = 100-(A+B) = 41{\cdot}50$$

$$100{\cdot}00$$

The average values for the "limiting point" deduced from the two methods jointly are consequently—

	1st Method.	2nd Method.	Mean.
Chloroform........	42·07	42·25	42·16
Water	16·18	16·25	16·21
Acetic acid........	41·75	41·50	41·63
	100·00	100·00	100·00

It is noteworthy that the proportion between chloroform and water thus deduced for the "limiting point" is close to that required for the molecular ratio $2CHCl_3$, $5H_2O$.

	Calculated.			Found.		
$2CHCl_3$......	239·0	=	72·64	42·16	=	72·23
$5H_2O$.......	90·0	=	27·36	16·21	=	27·77
	329·0		100·00	58·37		100·00

But whether this is merely an accidental coincidence, or is really due to a tendency to form a definite hydrate of chloroform in presence of acetic acid, cannot be decided by these experiments alone. The results with alloys subsequently described rather suggest that it is only a coincidence.

Alloys of Lead, Tin, and Zinc.

In Parts I and II seven different series of observations are recorded, made with mixtures of lead, tin, and zinc, at temperatures varying from about 565° C. up to near 800°, and containing lead and zinc in ratios varying from 2 : 1 to 1 : 2. On plotting these different series on the triangular system, it is evident that the critical curves deduced from those experiments where the temperature did not exceed 689—750° are substantially identical, whereas that derived from experiments at a higher temperature, 750—850° averaging near 800°, lies perceptibly *inside* the others. Again, but little discordance between the general directions and degrees of slope of the tie-lines is noticed in any of the series at temperatures not above 750° where the ratio of lead to zinc was 1 : 1 or 1 : 2; with the series where this ratio was 2 : 1, however, the upper ties do not closely coincide with those derived from the other series, but (as pointed out by Sir G. G. Stokes, Part IV) are inclined to them at angles not far from 5°, thus indicating the existence of some cause constantly at work interfering with separation into truly conjugate alloys; obviously this cause is the large preponderance of heavier alloy formed over lighter alloy, which, as above stated, produces a marked effect on the result by preventing thorough intermixture by stirring.

On plotting out in these seven cases the values of $\{A-A'-(B-B')\}^2$ as abscissæ, and of $A+A'$ and $B+B'$ as ordinates (Stokes' 2nd method, *supra*), similar differences are observed. All the experiments at 565—750° concord fairly well when the series where the ratio of lead to zinc was 1 : 1 or 1 : 2 are taken into account; whereas the series where the ratio was 2 : 1 exhibit a much wider departure. The experiments at 750—850°, and the concordant ones at 565—750°

lead to the following approximations respectively to the composition of the mixtures of metals at the limiting point:—

	565—750°		750—850°	
Lead......	21—23;	say 22	23—25;	say 24
Tin.......	34—36	„ 35	31—33	„ 32
Zinc......	42—44	„ 43	43—45	„ 44
		100		100

The critical curve for the higher temperature consequently lies *inside* that for the lower temperature, since it contains a limiting point corresponding with a smaller tin percentage.

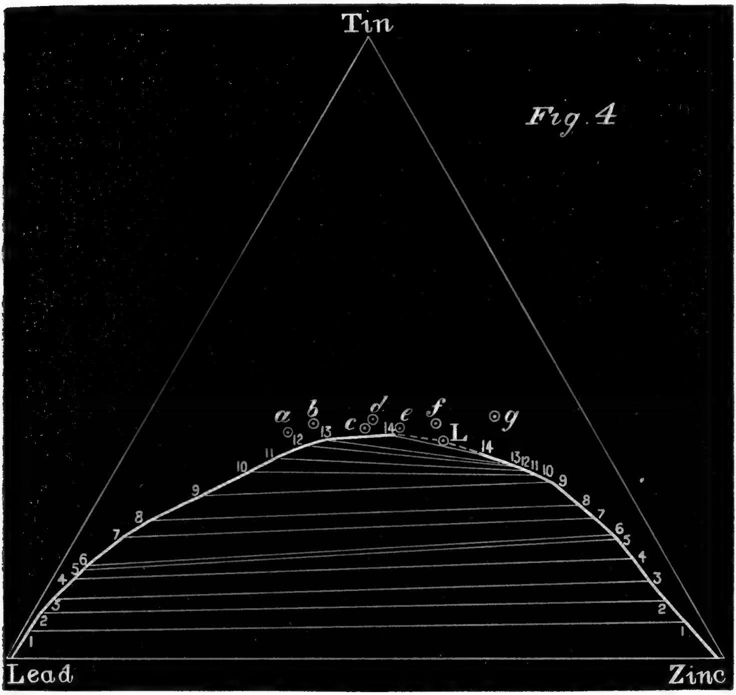
In order to define the upper portion of the critical curve still more exactly, and thus to deduce a closer approximation to the true limiting point, a number of additional experiments were made at a temperature close to 650°, with mixtures containing 31 to 34 per cent. of tin, and lead and zinc in proportions lying between 1 : 1·75 and 1 : 2, this being the ratio calculated to yield approximately equal quantities of heavier and lighter alloys, and thus to minimise the error due to imperfect intermixture. The following table gives the

No. tie-line.	Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
	Tin.	Zinc.	Lead.	Tin.	Zinc.	Lead.	
—	0	1·24	98·76	0	98·86	1·14	—
1	4·45	1·76	93·79	6·64	90·75	2·61	+2·19
2	6·94	2·05	91·01	9·91	86·71	3·38	+2·97
3	9·60	2·49	87·91	13·47	82·18	4·35	+3·87
4	12·60	3·66	83·74	16·68	78·48	4·84	+4·08
5	14·76	4·22	81·02	19·27	75·36	5·37	+4·51
6	16·11	4·73	79·16	20·35	73·51	6·14	+4·24
7	18·71	5·62	75·67	22·51	70·04	7·45	+3·80
8	21·95	7·57	70·48	25·07	66·72	8·21	+3·12
9	26·28	13·24	60·48	27·49	62·33	10·18	+1·21
10	29·43	16·76	53·81	28·64	59·94	11·42	−0·79
11	31·63	19·73	48·64	29·58	57·87	12·55	−2·05
12	33·88	22·36	43·76	30·33	56·25	13·42	−3·58
13	35·03	25·38	39·59	30·24	55·33	14·43	−4·79
14	35·65	35·25	29·10	32·85	48·51	18·64	−2·80

average values finally deduced from these experiments, together with the majority of the valuations derived from Series I, II, III, IV, and V (Part I), a few experiments in these series being discarded, together

with the entire Series VI where excess of lead was used, on account of showing less general concordance with the average than the others, obviously on account of incomplete intermixture and consequent formation of alloys not so truly conjugate as in the other cases. In all 88 compound ingots, and consequently 176 ternary alloys, were used in the deduction of the 14 pairs of conjugate points thus tabulated, so that each point represents the average of somewhat more than six alloy analyses.

Fig. 4 represents these values plotted on the triangular system.



On comparing the figures in the last column with the corresponding ones obtained in the previous experiments (Part I), it is noticeable that the tin distribution is such that the excess of tin percentage in the lighter alloy over that in the heavier one now shows *two* maxima, the second of which was not indicated by the former less complete series. The first is a + maximum, arrived at near the 5th tie-line; the second a — maximum, situated near to the 13th. Obviously the existence of a maximum (+ or —) is a necessary result of a slope

of the tie-lines to the one side or the other, since the lowest possible tie unites the points obtained with a percentage of solvent metal = *nil*, whilst the upper ties dwindle down to a point at the limiting point, so that for each of these limiting ties the percentage of solvent metal is the same in both heavier and lighter alloy formed; but it is noticeable that only in one other case out of a dozen ternary alloys now under examination is a *second* maximum noticeable, *i.e.*, only in this one other case (lead-aluminium-tin alloys) are the upper and lower ties found to slope in opposite directions; in all other instances, whether the ties slope to the right or to the left, the direction of the slope is the same throughout the whole extent of the critical curve.

It is further noticeable that both with lead-zinc-tin and lead-aluminium-tin alloys the position of the first maximum is such that it occurs when the ratio of lead to tin in the heavier alloy is sensibly near to that indicated by the formula $SnPb_3$. Thus in the case of the lead-zinc-tin alloys above described—

	Calculated.		Found.	
Sn	118 =	15·97	14·76 =	15·41
Pb_3	621 =	84·03	81·02 =	84·59
	739 =	100·00	95·78 =	100·00

The parallel results obtained with lead-aluminium-tin alloys will be described in a future paper; it may be noticed, however, that with neither series of alloys is any marked elevation or depression in the outline of the critical curve noticeable at the part corresponding with the compound $SnPb_3$, unlike the curve obtained with lead-zinc-silver and bismuth-sinc-silver alloys, where the formation of the definite compounds $AgZn_5$ and Ag_4Zn_5 leads to marked alterations of outline (*vide infra*).

It was found impracticable to obtain any accurate valuations of tie-lines situated nearer to the limiting point than No. 14; several attempts were made, but the results exhibited too great an amount of discordance amongst themselves, and too wide departures from the curve indicated by the above experiments, to enable them to be regarded as trustworthy; the causes of this being, as above stated, the slight difference in density between the two alloys formed, and the relatively large effect of temperature variation at this part of the curve as compared with other portions further removed from the limiting point. In fig. 4 (representing the above table plotted on the triangular system) L is the limiting point deduced by Stokes' 2nd Method from a carefully made large-scale plotting, giving as the most probable values $A+A' = 45$ and $B+B' = 85$, whence

$$\begin{aligned} \text{Lead} &= \tfrac{45}{2} = 22{\cdot}5 \\ \text{Zinc} &= \tfrac{85}{2} = 42{\cdot}5 \\ \text{Tin} &= \phantom{\tfrac{85}{2} =}\ 35{\cdot}0 \\ & \phantom{\tfrac{85}{2} =}\ \overline{100{\cdot}0} \end{aligned}$$

On account of the peculiar serpentine form of the curve obtained by the 1st Method, the positions of the limiting point cannot be so readily deduced by this method as in the case of chloroform, acetic acid, and water.

The points marked *a*, *b*, *c*, *d*, *e*, *f*, *g*, respectively indicate various "real" alloys obtained in the course of the experiments described in Parts I and II.

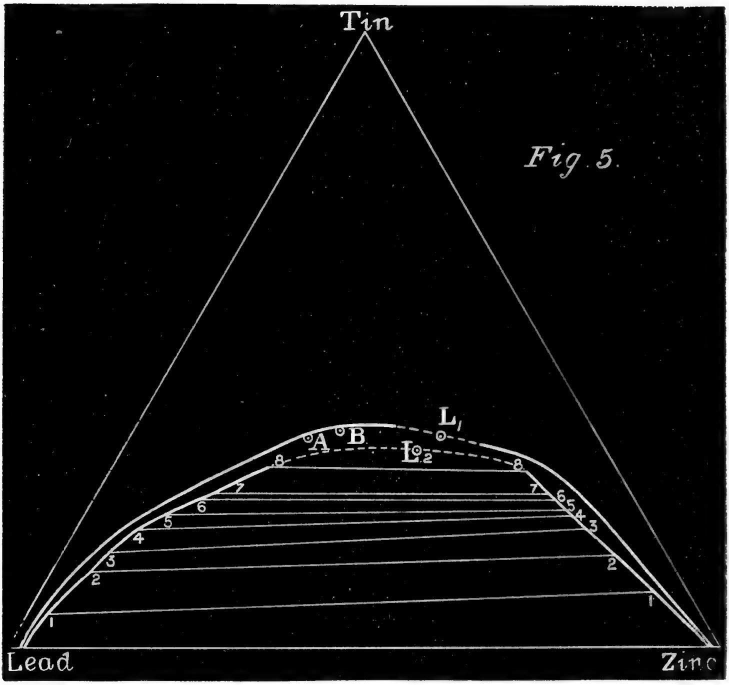
Critical Curve at 800°.

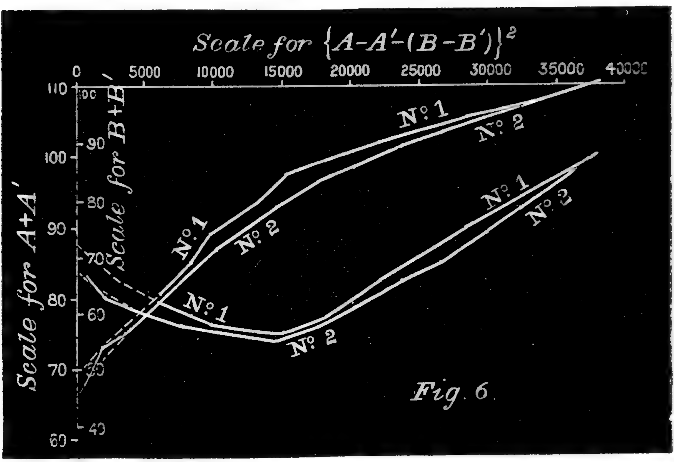
The experiments at temperatures lying between 750° and 850°, detailed in Part II, were similarly supplemented by some additional ones made with mixtures containing lead and zinc in the proportion 1 : 1·75 to 1 : 2, so as to obtain more accurate valuations, more especially in the case of the pairs of conjugate points lying nearer to the limiting point. The following table represents the average values finally obtained from these additional valuations conjointly with the former ones.

No. of tie-line.	Heavier alloy.			Lighter alloy.		
	Tin.	Lead.	Zinc.	Tin.	Lead.	Zinc.
—	0	98·70	1·30	0	1·57	98·43
1	6·51	89·97	3·52	9·56	4·20	86·24
2	12·04	83·09	4·87	15·80	7·13	77·07
3	16·31	78·17	5·52	19·84	8·87	71·29
4	18·81	74·02	7·17	21·79	10·64	67·57
5	21·29	69·12	9·59	23·80	10·60	65·60
6	24·60	61·77	13·63	25·33	12·30	62·37
7	26·43	57·07	16·50	26·44	12·15	61·41
8	29·12	50·51	20·37	28·47	12·64	58·89

Fig. 5 represents these values, the exterior curve indicating the numbers above stated for temperatures near to 650°. The points marked A and B represent two mixtures examined, not separating into two at 800°, although lying inside the critical curve for 650°.

On applying Stokes' 2nd Method, the curves marked No. 1, fig. 6, are obtained, those marked No. 2 being the corresponding curves deduced from the mean values at 650° above described. Obviously


Tin
Fig. 5.
Lead
Zinc
A
B
L1
L2


Scale for {A-A'-(B-B')}²
Scale for A+A'
Scale for B+B'
No. 1
No. 2
Fig. 6.

the curves marked 1 are closely in accordance with those deducible by shifting the curves marked 2 upwards on the paper; so that it may fairly be inferred that whilst the values of $A+A'$ and $B+B'$ at the limiting point are close to 45 and 85 respectively, those for curves No. 2 are not far from $A+A' = 48$, and $B+B' = 88$, as indicated by the dotted prolongations: whence, the compositions at this limiting point for 650° C. and 800° C. respectively are close to

	At 650°.	At 800°.
Lead	22·5	24·0
Zinc	42·5	44·0
Tin..........	35·0	32·0
	100·0	100·0

The points marked L_1 and L_2 (fig. 5) respectively indicate these compositions.

It is worthy of notice that in each case the ratio between lead and zinc is not far from that indicated by the formula $PbZn_6$.

	Calculated.	Found. At 650°.			Found. At 800°.		
Lead ..	34·67	22·5	=	34·62	24·0	=	35·29
Zinc ..	65·33	42·5	=	65·38	44·0	=	64·71
	100·00	65·0		100·00	68·0		100·00

A priori, it would seem not impossible that the position of the limiting point might correspond with the formation of a definite compound of the two immiscible metals, soluble in the "solvent" metal to an extent varying with the temperature. On the other hand, the experiments subsequently described show that when silver replaces tin as solvent metal an entirely different ratio between lead and zinc subsists at the limiting point thus found, more nearly approximating to Pb_2Zn. Experiments now in progress, moreover, indicate that with antimony as solvent metal a third ratio, approximately $PbZn_2$, is deduced.

It is further noticeable that the upper tie-lines, depicted in fig. 4 (Nos. 9 to 14), visibly converge together in such fashion that the point representing the composition $Sn = 28$, $Pb = 11$, $Zn = 61$ per cent. lies in the midst of them all. This configuration obviously suggests that there is a tendency to the formation of some definite compound, such that whilst a considerable range in composition is shown by the heavier alloys, a much smaller one is observed in the case of the lighter alloys conjugate therewith; that this compound is indicated by the formula $SnZn_4$ is strongly suggested by the circum-

stance that in this compound the two metals are to one another nearly in the ratio of that existing at this central point—

	Calculated for $SnZn_4$.	Found.		
Tin..........	31·2	28	=	31·5
Zinc..........	68·8	61	=	68·5
	100·0	89		100·0

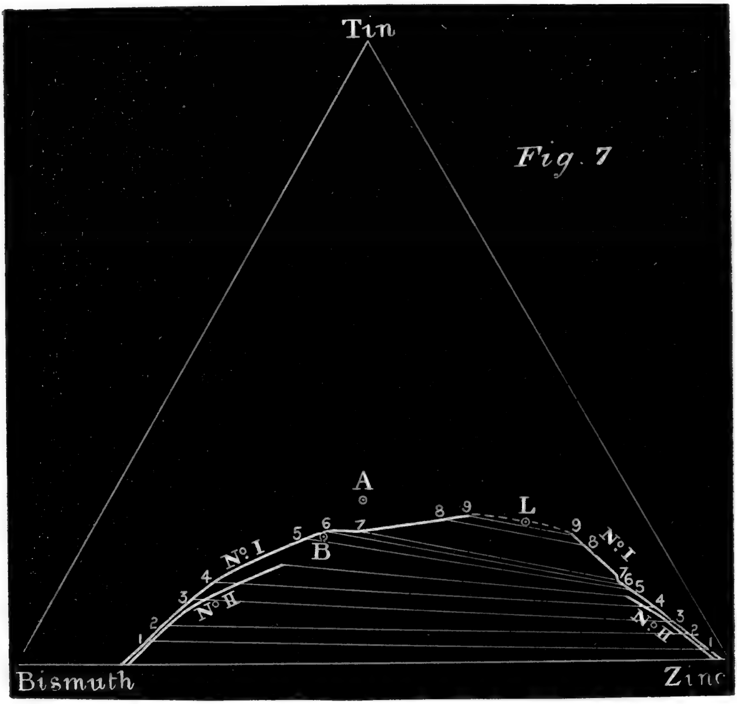
It will be shown in a future paper, that analogous convergent dispositions of the lines are observed with other ternary metallic mixtures, also leading to the conclusion that definite compounds are formed; a well marked case in point is noticed with alloys of lead, zinc, and cadmium, where a marked convergence is observable to a point where the cadmium and lead are in the ratio indicated by the formula $CdPb_2$.

Alloys of Bismuth, Tin, and Zinc.

The experiments described in Part III, when plotted on the triangular system, indicate that the limiting point in the critical curve for mixtures of bismuth, tin, and zinc is situated to the *right* (zinc side) of the central line of the triangle; so that in order to determine, with the least amount of error through imperfect intermixture, the conjugate points situated near the limiting point, the mixtures used must contain more zinc than bismuth. Accordingly a number of additional experiments were made with mixtures containing bismuth and zinc in the proportion 1 : 2·5 to 1 : 3·5, and from 12 to 21 per cent. of tin. The figures thus obtained showed that some of the compound ingots previously examined (prepared with equal quantities of bismuth and zinc) were slightly but sensibly affected by the error due to incomplete intermixture, owing to the preponderance of heavier alloy formed over the lighter one. The following table exhibits the average values finally deduced from these further experiments, together with those of the former series not materially affected by this source of error, at temperatures averaging near to 650° C. It is noticeable that these further experiments show that the numerical value of the excess of tin percentage in the lighter alloy over that in the heavier attains a maximum (of negative sign), and then diminishes again; a point not shown by the less complete series of values described in Part III.

Fig. 7, curve No. 1, represents these values, No. 2 representing the analogous figures obtained at 700°—800° (Series II, Part III). Obviously the critical curve for the higher temperature lies *inside* that for the lower temperature, as in the case of the lead-zinc-tin alloys above described. The point marked A represents a mixture not sepa-

No. of tie-line.	Heavier alloy.			Lighter alloy.			Excess of tin percentage in lighter alloy over that in heavier.
	Tin.	Bismuth.	Zinc.	Tin.	Bismuth.	Zinc.	
—	0	85·72	14·28	0	2·32	97·68	0
1	3·23	80·27	16·50	1·98	3·45	94·57	−1·25
2	6·35	75·82	17·83	3·97	4·21	91·82	−2·38
3	10·38	70·52	19·10	6·35	5·53	88·12	−4·03
4	13·53	66·33	20·14	7·95	6·51	85·54	−5·58
5	19·09	51·35	29·56	11·19	8·68	80·13	−7·90
6	20·80	46·01	33·19	12·72	9·72	77·56	−8·08
7	20·44	42·43	37·13	12·98	9·70	77·32	−7·46
8	21·89	30·33	47·78	17·25	11·92	70·83	−4·64
9	22·37	25·94	51·69	19·16	13·04	67·80	−3·21



rating into two at 650° ("real" alloy); that marked B, a similar mixture, not separating at 750°; these contained respectively (average of analyses of both ends):—

	A.	B.
Bismuth..........	38·7	48·2
Zinc.............	35·3	31·8
Tin..............	26·0	20·0
	100·0	100·0

The point B evidently falls well outside the critical curve for 750°, although it is not distinctly outside that for 650°.

On applying Stokes' second system to the above values, the following figures result from a large-scale plotting:—$A + A' = 37$, $B + B' = 120$; whence the composition for the limiting point, L, is:—

Bismuth..........	18·5
Zinc.............	60·0
Tin..............	21·5
	100·0

These percentages of bismuth and zinc are not far from those required for the formula $BiZn_{10}$.

	Calculated.	Found.		
Bismuth........	24·4	18·5	=	23·5
Zinc...........	75·6	60·0	=	76·5
	100·0			100·0

Inasmuch, however, as an entirely different proportion is found when silver is the "solvent" metal (approximately $BiZn_2$), this cannot be regarded as much evidence of the existence of an atomic compound of bismuth and zinc at the limiting point.

On contrasting together the curves thus deduced for mixtures of lead-zinc-tin and bismuth-zinc-tin, it is obvious that in each case the curve for a higher temperature lies *inside* that for a lower temperature. When bismuth is the heavier immiscible metal, the curve lies inside that obtained with lead instead of bismuth; apparently the same relationship holds in the case of $\left.\begin{matrix}\text{bismuth}\\\text{lead}\end{matrix}\right\}$-zinc-silver alloys and of $\left.\begin{matrix}\text{bismuth}\\\text{lead}\end{matrix}\right\}$-aluminium-tin alloys, as will be hereafter discussed.

The limiting points above deduced in each case lie to the *right* (zinc side) of the central line of the triangle, and the uppermost tie-lines in each case slope to the right. With bismuth-zinc-tin alloys the same disposition is also observed with the lower tie-lines, but with lead-zinc-tin alloys the lower ties slope to the left, the reason for this difference probably being that lead and tin exhibit a tendency to combine together to form a definite compound, $SnPb_3$, the formation

of which affects the composition of certain of the heavier alloys, whilst no similar compound appears to be formed between bismuth and tin. Owing to the critical curve for bismuth-zinc-tin being so much lower down than that for lead-zinc-tin, the tendency towards formation of the compound $SnZn_4$, which affects the direction of slope of the ties in the latter curve, is almost invisible with the former ones. Some slight indication of an effect is, however, perceptible, the slope of the ties to the right visibly ceasing to increase towards the upper part of the curve in the same marked way as is noticeable at the lower part thereof. It is remarkable in this connexion that the upper ties with alloys of lead and zinc, when tin is the solvent metal, are the only ones sloping downwards to the right, those obtained with lead and zinc when silver, cadmium, or antimony is the solvent metal, uniformly sloping downwards to the left. Precisely the same remarks apply to the corresponding alloys containing aluminium instead of zinc so far as at present examined.

Alloys of Lead, Silver, and Zinc.

The experiments detailed in Part II indicate that the position of the limiting point with lead-silver-zinc alloys lies considerably to the *left* of the central line of the triangle, and not to the right as with the above-described alloys containing tin. Accordingly, a number of additional experiments were made with mixtures containing lead and zinc in proportions from 3 : 2 to 4 : 1 (according to the amount of silver present), with the object of correcting any possibly erroneous values due to imperfect intermixture owing to inequality between the quantities of heavier and lighter alloys formed; and also of obtaining pairs of conjugate points situated nearer to the limiting point than those already deduced, and thus of enabling the position of the limiting point itself to be calculated. In all these further experiments the percentage of silver in the mixture exceeded 20, so that only those points were re-valued lying nearer to the limiting point than those where the effect of the formation of the compound $AgZn_5$ in producing irregularity became appreciable. The temperature was 750°—850° throughout, averaging near to 800° as before.

The following table represents the average results obtained by taking into consideration these further experiments, together with those described in Series I and II, Part II, none of which were found to be affected by the error due to incomplete intermixture to anything like so great an extent as was found with some few of the ingots prepared with tin. The 14 pairs of conjugate points are deduced from the examination of 39 compound ingots in all, representing 78 ternary alloys. Each point, therefore, nearly represents the average of three alloy analyses.

No. of tie-line.	Heavier alloy.			Lighter alloy.		
	Silver.	Lead.	Zinc.	Silver.	Lead.	Zinc.
1	1·25	96·69	2·06	38·91	3·12	57·97
2	1·54	96·28	2·18	40·89	3·38	55·73
3	1·71	96·43	1·86	45·01	3·37	51·62
4	2·39	95·78	1·83	47·68	3·74	48·58
5	4·18	94·43	1·39	52·80	4·09	43·11
6	5·55	93·16	1·29	54·93	4·21	40·86
7	10·22	88·02	1·76	60·14	9·00	30·86
8	12·62	85·38	2·00	63·70	11·30	25·00
9	15·69	81·88	2·43	65·34	13·87	20·79
10	17·43	80·15	2·42	65·94	14·79	19·27
11	17·65	79·78	2·57	67·03	16·48	16·49
12	19·51	78·54	1·95	63·79	22·29	13·92
13	29·53	68·03	2·44	60·35	28·42	11·23
14	29·90	67·21	2·89	59·32	29·15	11·53

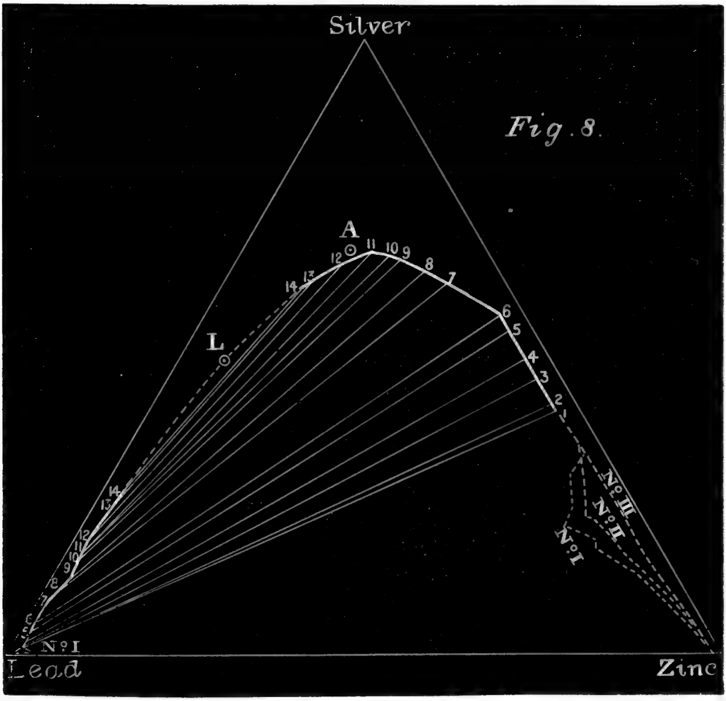


Fig. 8 represents these values on the triangular system, the point A indicating an alloy that did not separate, containing Ag = 67·5, Pb = 20·1, Zn = 12·4.

The position of the limiting point L is found by a large-scale plotting on Stokes' second system to correspond with the values:—

$$A+A' = 96{\cdot}4$$

$$B+B' = 12{\cdot}6$$

whence the composition at the limiting point is:—

Lead	=	48·2
Zinc	=	6·3
Silver	=	45·5
		100·0

It is noteworthy that the ratio between lead and zinc at the limiting point thus found with silver as the "solvent" metal, is entirely different from that deduced above when tin is the solvent; with tin, the ratio corresponds pretty closely with that indicated by the formula $PbZn_6$, whereas with silver it corresponds more nearly with Pb_2Zn.

	Calculated.	Found.		
Pb..........	86·43	48·2	=	88·44
Zn..........	13·57	6·3	=	11·56
	100·00	54·5		100·00

The dotted lines I, II, III at the lower portions of the critical curve represent the values described in Series I, II, and III respectively (Part II), and indicate very clearly the effect produced by the formation of the definite compound $AgZn_5$, and its gradual elimination as previously described. Similarly, the effect of the formation of the compound Ag_4Zn_5 is readily visible in the right-hand branch of the curve; the conjugate point No. 6 is obviously close to an angle in the curve line and represents an alloy containing silver and zinc in the proportions 54·93 and 41·86, or almost exactly Ag_4Zn_5.

	Calculated.	Observed.		
Silver......	57·07	54·93	=	56·75
Lead.......	42·93	41·86	=	43·25
	100·00	96·79		100·00

Leaving out of sight the bulge inwards at the lower part of the curve, due to the formation of $AgZn_5$, it is noticeable that the critical curve for lead and zinc with silver as "solvent" metal lies *outside* that

with tin as solvent. As shown below, the same relationship also holds with bismuth-zinc-silver and bismuth-zinc-tin curves.

Alloys of Bismuth, Silver, and Zinc.

The experiments described in Part III show that with bismuth-zinc-silver alloys, as with those of lead-zinc-silver, the position of the limiting point lies to the *left* (bismuth side) of the central line. Accordingly, a number of additional experiments were made with silver percentages upwards of twenty, and with zinc and bismuth in the proportions 1 : 1·33 to 1 : 1·5, so as to deduce more accurately and completely the upper portion of the critical curve, only those points being re-valued where the influence of the formation of the compound $AgZn_5$ had become inappreciable.

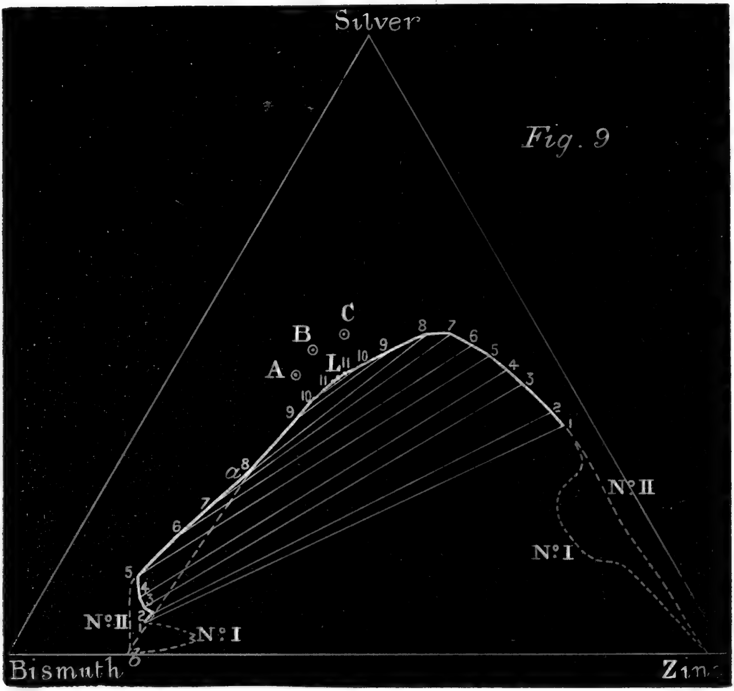
The following table exhibits the average results thus obtained, together with those described in Series I (Part II), the temperature throughout being 700°—800°, averaging near to 750°; twenty-four compound ingots in all being employed, representing forty-eight ternary alloys.

No. of tie-line.	Heavier alloy.			Lighter alloy.		
	Silver.	Bismuth.	Zinc.	Silver.	Bismuth.	Zinc.
1	5·08	79·28	15·64	37·06	5·34	57·60
2	6·84	77·24	15·92	38·80	5·93	55·27
3	7·58	78·58	13·84	43·16	7·99	48·85
4	8·98	77·99	13·03	46·31	8·67	45·02
5	12·71	76·19	11·10	47·94	10·05	42·01
6	18·68	68·97	12·35	49·92	12·42	37·66
7	22·96	62·72	14·32	51·34	14·34	34·32
8	29·26	53·10	17·64	51·16	17·21	31·63
9	37·33	43·27	19·40	49·73	22·78	27·49
10	41·22	38·52	20·26	47·15	28·76	24·09
11	43·74	33·82	22·44	45·25	32·11	22·64

Fig. 9 represents these results on the triangular system, the position of the limiting point L being thence deduced by Stokes' second system, a large-scale plotting giving the values :—

$$A + A' = 66{\cdot}25$$
$$B + B' = 44{\cdot}5$$

whence the composition at the limiting point is :—



Bismuth	33·12
Zinc.............	22·25
Silver	44·63
	100·00

This represents a ratio between bismuth and zinc not far from that indicated by the formula $BiZn_2$; whereas the corresponding ratio with tin as solvent metal was found to be not far from that indicated by $BiZn_{10}$.

	Calculated.	Found.		
Bismuth....	61·76	33·12	=	59·82
Zinc	38·24	22·25	=	40·18
	100·00	55·37		100·00

The dotted lines I and II at the base of the critical curve represent respectively the values given in Series I (Part III), obtained by fusion for eight hours, and in Series II and III, obtained by separate further fusion of the lighter and heavier alloys thus formed. Like

the corresponding results obtained with lead-zinc-silver alloys shown in fig. 8, these lines clearly indicate the effects due to the formation of $AgZn_5$ and Ag_4Zn_5; the formation of the first leading to a marked bulge *inwards* on both sides of the curve at the lowest part; and that of the second to an *outward* bulge somewhat higher up in the left-hand branch, as compared with the dotted line *ab* connecting the upper part of the curve with the point *b* on the base line, representing the alloy of bismuth and zinc formed in the absence of "solvent" metal.

The points marked A, B, and C represent certain mixtures examined that did not separate into two different alloys, *i.e.*, that formed "real" alloys at 750°.

Leaving out of sight the lower bulges inwards and outwards, due to formation of $AgZn_5$ and Ag_4Zn_5, it is evident that the critical curve for bismuth-zinc-silver lies *inside* that for lead-zinc-silver, just as that for bismuth-zinc-tin lies inside that for lead-zinc-tin. Further, the curve for bismuth-zinc-silver lies *outside* that for bismuth-zinc-tin just as the curve for lead-zinc-silver lies outside that for lead-zinc-tin. With silver as "solvent" metal the limiting point lies to the *left* of the central line of the triangle (bismuth or lead side); whilst with tin as solvent metal it lies to the right (zinc side). In the first case the ties uniformly slope downwards to the left, and in the second, to the right so far as the upper ones are concerned; although a slope to the left is observed with the lower ones for the lead-zinc-tin alloys, suggesting the existence of a tendency to form a definite compound of lead and tin (possibly $SnPb_3$), no analogous tendency being noticeable with bismuth and tin.

The author has much pleasure in acknowledging the assistance afforded him by Mr. Sydney Joyce in carrying out the further experiments above described.

IV. "Note on some Specimens of Rock which have been exposed to High Temperatures." By Professor T. G. Bonney, D.Sc., LL.D., F.R.S. Received December 18, 1891.

The effects of raising several varieties of rock to temperatures which, though high, in some cases were below those required to produce fusion were described to the Royal Society by Mr. Rutley in 1886.* The following notes may be useful as a continuation of a subject which is not without interest owing to its bearing on natural processes.

Some time since, Mr. J. Postlethwaite, F.G.S., of Keswick, kindly

* 'Roy. Soc. Proc.,' vol. 40, p. 430.

presented me with two "vitrified" specimens of the so-called quartz-felsite of St. John's Vale, together with some fragments of the ordinary rock. All had been obtained from the quarry near Threlkeld. The vitrified specimens, as he informed me, "had been thrown with some fuel into the fire of the engine which propels the machinery by which the felsite is crushed for road-metal." They had been picked out from amongst the engine-slag, and had been given to him by Mr. R. Humphreys, the late foreman of the quarry. Mr. Postlethwaite subsequently, on enquiry, at my request, was told that the temperature of the fire would probably be not less than that of a large locomotive (the engine is 70-horse power), in the hottest part of which pig-iron melts. So that these specimens may have been raised to a temperature of about 2000° F.*

The rock is intrusive in Skiddaw slate, fragments of which it includes, and which it slightly alters near the junction. It is so well known to geologists that we may refer for most particulars to the Survey map and the memoir,† in which are given a brief description (p. 33) and a small figure of the microscopic structure (Plate I, fig. 5). The rock is a somewhat porphyritic micro-granite; the matrix is holocrystalline, consisting chiefly of small grains of felspar and quartz. The former are usually rather too much decomposed to admit of the species being determined; as a rule, they appear to have consolidated before the quartz, since they are rectilinear (nearly square) in outline, the other mineral filling the interspaces. The quartz contains several minute fluid cavities, with relatively small bubbles, and an occasional microlith or belonite, so small that any attempt to determine it would be a waste of time. A fair number of mica flakes, which sometimes run to a larger size, are also present. Of these, the smaller occasionally appear to be moulded to the felspars. Evidently they once belonged to the biotite group, but are now replaced, as is very common, by a pale-greenish, slightly dichroic, secondary mineral—a hydrous mica, or possibly a chlorite. There are sparse granules of iron oxide and of (?) impure sphene.

In this ground-mass are scattered larger felspar crystals, also more or less decomposed, in which sometimes the striping of plagioclase can still be distinguished, but at others the crystal is occupied entirely by a minute fibrous or filmy secondary product, affording

* Dana, 'Characteristics of Volcanoes,' p. 144, considers that the temperature of molten basalt, such as that of Kilauea, is from 2000° to 2500° F. For complete fusion of such a rock as the St. John's Vale quartz-felsite a higher temperature would be required than in the case of basalt, but, as the Hawaiian lava is very liquid, and some basic rocks, when still melted, are below 2000°, perhaps the latter of these temperatures might be sufficient.

† 'Mem. Geol. Survey,' Lake District (by J. C. Ward), p. 8. See also a paper by the same author, 'Geol. Soc. Quart. Jl.,' vol. 32, pp. 12, 22, &c.

rather brilliant tints with the crossed nicols. The crystals are occasionally composite, some are fairly idiomorphic, others appear to have suffered more or less external corrosion. There are besides some grains of quartz (also affected by the action of the magma) which contain fluid cavities with a rather stream-like arrangement. They are similar to, but generally larger than, those in the ground-mass. Both minerals occasionally enclose microliths, among them, I think, zircon. Besides the mica, we find one or two rather tufted groups of a prismatic mineral, which exhibits a fairly marked dichroism, pale-brown to bluish-green, at first sight not unlike tourmaline. As, however, the extinction certainly is oblique, though the angle is not large, this mineral is no doubt a hornblende, and probably of secondary origin.

The account may be completed by quoting the following bulk analysis of the rock, given by Mr. Ward ('Geol. Soc. Quart. Journ.,' vol. 32, p. 22) :—

SiO_2	=	67·180
Al_2O_3	=	16·650
Fe_2O_3	=	0·559
FeO	=	2·151
CaO	=	2·352
MgO	=	1·549
K_2O	=	2·914
Na_2O	=	4·032
P_2O_5	=	0·179
SO_3	=	trace
CO_2	=	0·885
Loss on ignition	=	1·549
		100·000

The vitrified specimens are coated externally with a pellicle of dark-brown glass; the surface of one being rather scoriaceous, and "pitted" with cavities full one-third of an inch in diameter; that is, the ordinary surface of broken felstone (for I presume this to be the outside of a fragment) is replaced by that of a vesicular slag. Internally many minute cavities, from the size of a small pin-head downwards, have been developed; the rock has assumed a darker gray colour, in which the white porphyritic crystals of felspar stand out more distinctly than before;* and it has a more scoriaceous aspect.

On examining the slides cut from the partially melted rock, two

* In the specimens of unaltered rock the larger crystals of felspar are of a yellowish tint; the smaller, however, are white, but less distinct, owing to the pale gray matrix.

changes are obvious at a glance—the development of a number of vacuoles,* and the presence of a considerable amount of brown glass. The former are usually spherical, or nearly so, with well-defined, "clean-cut" edges. The smaller of them are often lined, perhaps in some cases partly filled, with opacite, and are thus rendered more or less opaque. In some cases, very tiny clear crystallites are interspersed among the blackest dust. The ground-mass of the rock consists of somewhat rounded, or sub-angular, grains of a clear mineral set in a base of light-brown glass. Of these, the majority unquestionably represent the quartz of the original ground-mass, but I think that some are felspar. Most of the mica has also vanished, though here and there a small remnant may be detected. The cavities in these quartz grains appear to me to be less numerous but a little larger than before. Often they are seemingly empty, but in some the bubbles still remain apparently without any change. The larger quartzes of the unaltered rock now appear as grains which have a rather irregular outline, and are much cracked. Here also the cavities seem to be reduced in number, but not to have increased in size. The larger felspars have disappeared; but their position, on closer examination, can be identified by irregular patches of dirty glass, in which are very numerous vacuoles; these generally are rather small in size, and coated with dark dust. In parts of the glass minute fibres may be observed, which, however, do not act on polarised light; in others a tiny patch or spot is feebly doubly-refracting. In other parts (especially, I think, where the vacuoles are larger, but less numerous) the glass is a deep-brown, barely translucent, or even opaque. Here and there, in the glass generally, and near the edges of included grains, groups of minute vermicular cavities appear, and a few microliths occur in the slide, which are so small that it would not be safe to offer an opinion as to their nature or age.

Practically, the effect of the heating has been to melt down the felspathic and the micaceous constituents, without very materially affecting the quartz, and to render the mass vesicular.

At Les Talbots, in Guernsey, brick is made of a material which is mainly, if not wholly, disintegrated granite. An over-burnt specimen which I have examined is a brown slaggy glass, full of mineral fragments.† These, under the microscope, are seen to be chiefly quartz and felspar. The grains of the former are traversed by numerous cracks, and the pieces appear to have been sometimes displaced, but to be otherwise unaffected. Those of felspar also are cracked. Numerous vesicles have formed: the majority very small, but some are easily

* These, it will be remembered, were developed in the experiments described by Mr. Rutley, both in a pitchstone from Arran and an obsidian from Montana.

† The specimen was collected by the Rev. E. Hill, F.G.S. In 1888 he took me to the locality.

seen with a low power; these are more or less coated with opacite, and set in a glass variable in character; in parts it is rather deep-brown, in others fairly clear, almost colourless, but crowded either with very minute and rather filmy microliths or with pale-coloured belonites and granules of opacite (the last being sometimes in more considerable quantities than one would have expected); now and then it is simply a brown glass. In one fragment with multiple twinning the lines indicating the composition faces can be traced in parts which have ceased to act on polarised light. In some fragments, mainly vitrified, spots occasionally may be observed which continue to be doubly refracting, though rather feebly, appearing like faint white clouds on a dark background. The ground-mass is studded with minute fragments of quartz, perhaps with some felspar, set in a glass full of cavities, the smaller being lined with opacite, as in the case of the last-described rock.*

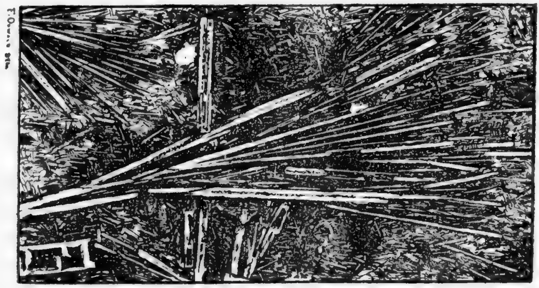
Nearly forty years since, an attempt was made by Messrs. Chance, of Birmingham, to utilise basalt for various purposes, by melting it and then running it into moulds, pouring it out in sheets, &c. Reference has often been made to these experiments, but, so far as I am aware, no detailed account was published, nor has any connected study been made of the results.† It may be, therefore, of interest to record the structure of some specimens which I have examined. Two of these were given to me a short time since by J. T. Chance, Esq. One of them is a large fragment, evidently part of a block which has been cast in a mould. It was labelled, "melted 1851 to 1854, devitrified."‡ On the surface which was in contact with the mould is a film of deep-brown glaze, like that of an over-burnt Staffordshire "blue-brick." The rock is dark, almost black, with a faint purple tinge, generally compact, exhibiting slight traces of a crystalline structure in one part. In it are a fair number of small cavities, more or less spherical, and a few of rather large size, towards the exterior, very irregular in shape. On microscopic examination the following constituents are observed:—(1) magnetite, very abundant in minute grains, which, however, evidently are often cubes or octahedra; (2) very numerous microlithic prisms, ranging up to about 0·002 inch in

* Mica should be present, but I only find two flakes, and these are so much altered as to be barely recognisable, having lost their characteristic dichroism, and looking like bundles of yellowish fibres.

† They are referred to for comparative purposes by Messrs. Judd and Cole in their important paper on the Basalt-glass (tachylyte) of the Western Isles of Scotland ('Geol. Soc. Quart. Jl.,' vol. 39, p. 444); also by Mr. Waller, 'Midland Naturalist,' vol. 8 (1885), p. 261.

‡ From 'Ure's Dictionary of Arts,' &c., *s.v.* "Basalt," it would appear almost certain that this specimen was "devitrified" by slow cooling, obtained by running the molten material into moulds of sand contained in iron boxes, raised to a red heat.

length;* (3) thin microliths of felspar, not seldom about 0·01 inch long, sometimes as much as 0·02 inch. These, from measurement of the extinction angles, may be referred, at any rate in many cases, to labradorite. They may be called skeleton crystals, for not unfrequently the edges are penetrated by microliths of the other minerals, or they enclose a sort of thin bar composed of them; in some cases the outline of the latter is a very acute-angled triangle, and a few microliths (usually of the augite) are scattered in advance of the apex along a line in the crystal. These crystals have a tendency to



"Devitrified" basalt, showing skeletal crystals of felspar in a ground-mass full of crystallites of the same. × 33.

bifurcate, and to form slightly divergent groups, as shown in the illustration.† The magnetite, also, is often grouped along lines which run at right angles, one to another, as figured by Vogelsang‡ and by Zirkel.§

The ground-mass among the skeletal crystals consists of a confused aggregate of the same constituents, but of smaller size, so that they cannot be readily distinguished with powers below a one-eighth objective. It is difficult to be quite certain, but I think that no glass remains. The supposed augite is sometimes slightly vermicular in form. The felspar appears to have consolidated last, and the same, I think, was the case with the larger skeleton crystals, though the mass when they formed must have been sufficiently plastic to allow these to be developed in definite directions for some distance. Mr. Teall's figure|| of the Cleveland dyke, from near Preston, presents a general

* It is difficult to determine their exact nature, but I have little hesitation in identifying them with augite.

† A somewhat similar, but more curved and variable, grouping in the case of augite is described by E. S. Dana in a compact basalt from Mount Loa, Sandwich Islands.—J. D. Dana, 'Characteristics of Volcanoes,' p. 320, &c.

‡ In a basalt from near North Berwick ('Die Krystalliten,' Plate XIII, fig. 1).

§ 'Basaltgesteine,' fig. 54.

|| 'Geol. Soc. Quart. Jl.,' vol. 40, Plate 12, fig. 4.

resemblance to the subject of our description, except that in it the crystals are longer, thinner,* and differently grouped, and a slightly-curved arrangement is perceptible in the ground-mass.

The second specimen is a large fragment of a slab of black glass, about one-third of an inch thick. In a thin slice this appears, on microscopic examination, to be a brown glass, which becomes quite opaque in a narrow zone† near each edge, but in the intermediate part varies from a light-brown to a rather rich umber-brown, each tint being slightly granulated. It exhibits a fluidal structure, the stripes in one part being bent into a fold. The use of high powers brings out specks of opacite, but fails to resolve the glass, so that obviously the colouring matter is very finely distributed.

A third slide, cut from a specimen given to me by Professor Judd, shows well-defined spherulites‡ (deep-brown) in a glass (pale-brown, almost buff). In the centre of some of these spherulites is a cruciform group consisting of aggregated granules of opacite or ferrite, from which radiate similar but thinner bands, interspersed with clearer fibres, which also have a tendency to group themselves crosswise, rather than to diverge in all directions. These fibres seem to have a very faint depolarising influence, but they are so small and still so discoloured as to be unfit for examination.§ These fibrous structures can generally be traced to the edge of the spherulite, but the dark lines, by which alone it is to be recognised, become so thin that the outer part, with low powers, appears commonly to be structureless.

A fourth specimen in the museum at University College is labelled "Melted Rowley Basalt, cooled in 13 days; from upper surface. Eagle Foundry, Birmingham, February, 1836."‖ This is a black glass, with a slightly irregular, sub-conchoidal fracture, rather vesicular in one part, and opposite to this bearing apparently the impression of a mould. Under the microscope, it is a clear glass of a warm-brown colour, by no means dark. It contains a few minute cavities and granules of opacite; there are some larger vacuoles, circular in section, from 0·01 inch to 0·05 inch in diameter, and a few circular spots, about the same size, slightly grayer and more granular than the rest of the rock; also one small portion of a deeper brown glass

* Though occasionally one more "stumpy" may be seen.

† Mr. Rutley observed that an opaque zone occurred in a specimen of vitrified basalt where it joined a fragment of unmelted basalt, which had been dropped into the crucible after this had been removed from the flame.

‡ Mr. Rutley observed a rudely spherulitic structure in a slowly cooled specimen of melted basalt (Plate V, fig. 4).

§ In the immediate neighbourhood of the central crosses they seem to enclose ferrite granules, arranged along lines with very short cross-pieces.

‖ The date is as printed, but, as each label bears the same signature, "William Hawkes," it may be a clerical error for 1856.

with a paler border, something like a fleur-de-lys in shape. But on the whole the glass is remarkably uniform and free from microlithic enclosures of appreciable size.

A fifth specimen consists of a number of small black glassy fragments labelled "Melted Rowley Basalt, cooled in water. Eagle Foundry, Birmingham, February, 1858." This material is not suited for slicing, so I have had some of it pounded and have examined the powder. This shows the rock to be a glass which breaks into rather flat sharp-edged chips with a clean somewhat conchoidal fracture; in colour a translucent umber-brown, not dark in the thicker parts of the fragments, and a very faint olive-brown, almost colourless, in the thinner. One or two of the larger chips give slight indications of a fluidal structure, and contain, rarely, a minute cavity or a granule or two of opacite.

All the basalt on which Messrs. Chance experimented came, I believe, from the well-known mass near Rowley Regis,* which has been often described. The natural rock is usually fairly coarsely crystalline; but I happen to possess specimens of an unusually glassy variety, collected by myself nearly twenty years since, from a pit which was then being opened on the northern side of the hill, near some collieries, which, as I was told, were called the California pits, the excavation being evidently almost at the base of the basalt. These specimens, macroscopically, are compact dark magma-basalts, with rather smooth sub-conchoidal fracture, one† slightly more compact than the other. Under the microscope we see in both, but more commonly in the less compact, sparse grains of serpentinised olivine and crystals of plagioclase, each about 0·003 inch long, together with grains of iron oxide (magnetite, or perhaps in some cases ilmenite) scattered in a colourless matrix which is thickly studded with smaller granules of iron oxide, and tiny, rather irregularly formed prisms of almost colourless augite, among which the presence of lath-like microliths of felspar is suggested. On applying polarised light (with a magnification of about 50 diameters) much of the clear part seems not to produce any depolarising effect, though obviously felspar microliths are frequently present in it; but with about 150 diameters very faint bands of gray light make their appearance —most clearly in the coarser-grained specimen: in the other, even when a quartz plate is used, no difference of tint can be seen in some parts, either as the stage is rotated, or by comparison of the edge of the slice with the coloured field. The indefinite, almost ghost-like

* See Allport, 'Geol. Soc. Quart. Jl.,' vol. 30, p. 548. Mr. Rutley described specimens of melted basalt from the Giant's Causeway, and from Kilauea and Mokua Weo-Weo, in the Hawaiian Islands.

† The first obtained, for I visited the quarry a year or two afterwards, when it had been considerably enlarged.

aspect of the crystallites (where they are seen) does not appear to proceed from a crowding of minute microliths (for a crystallite may sometimes be larger than one of the augite granules), but from its power to depolarise being weak, as if its molecular condition were only slightly anisotropic. It is, therefore, possible that the rock is in the act of passing rather than has actually passed from a glassy condition, though it is not truly a tachylite.

On comparing the results of artificial processes, described above, with rocks which have solidified from natural fusion, we observe (1) that while quartz and felspar crystals in acid rocks, after having formed, frequently appear to have been partially melted down, the quartzes, as a rule, are not cracked, the inner parts of the felspars are not locally converted into glass. The exterior only—as when a soluble substance is acted on by a fluid—appears to have been affected; (2) that, in the case of the basic rocks artificially melted, the glassy part is a true tachylite, but the structure of the devitrified specimen, with its peculiar skeletal crystals of felspar and magnetite and absence of well-defined augite, is unusual in nature.* It is, however, as I know from specimens in my own collection, and as may be seen from Vogelsang's book, rather characteristic of slags and glasses. This appears to be confirmatory of the view commonly entertained that an igneous rock is not liquefied by the action of heat alone, but that water is a contributing agent, and is always present in the magma; further that the formation of a glass in the process of cooling is facilitated by the escape of the water, which may explain the comparative rarity of tachylites in nature, and the fact that when they do occur, they are very seldom more than "selvages" to masses of basalt.

Presents, January 28, 1892.

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* I find a slight approach to it in some vesicular basalts, *e.g.*, one or two from the Sandwich Islands. See also the figures in Messrs. Judd and Cole's paper on Scotch Basalt-glass, *loc. cit.*, Plates XIII and XIV.

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February 4, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

The Right Hon. Farrer Herschell, Baron Herschell, was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the New Star in Auriga. Preliminary Note." By J. NORMAN LOCKYER, F.R.S. Received February 4, 1892.

From a note in 'The Times' of Wednesday, February 3rd, I learnt that a new star had been discovered in the constellation Auriga, and that photographs had been obtained at Greenwich on Monday night.

Observations were therefore impossible here before last night. This is much to be regretted, and suggests that some local organisation is needed to further quick transmission of news to observing stations relating to phenomena which may change in a few days or even hours.

Last night was fortunately fine, and two photographs were taken of the spectrum:—

	h.	m.	
The 1st exposed........	1	30,	from 7.30 to 9
The 2nd ,, ...	3	0	,, 9.30 ,, 12.30

The first registered thirteen lines; the second appears to contain some additional ones, but they are very faint and have not yet been measured.

A complete discussion of these photographs will form the substance of a subsequent communication, but already the following approximations to the wave-lengths have been obtained, the photographs being treated absolutely independently, means, however, being taken for the four least refrangible lines, as there has not yet been time to construct a proper curve for this region.

I have recently taken up the question of stellar spectra, and find that a 6-inch object glass, with a prism in front of it, is all that is required for the brighter stars. This instrument was employed upon the nova, which is of about the 5th magnitude, so the exposures were necessarily long.

Lines Measured in the First Photograph.

Wave-length.	Hydrogen lines.	Probable origin.
3933 (K)	..	Ca
3968	H	
4101	*h*	
4128		
4172		
4226	..	Ca
4268		
4312	..	Hydrocarbon
4340	G	
4516		
4552		
4587		
4618		

Lines Measured in the Second Photograph.

Wave-length.	Hydrogen lines.	Probable origin.
3933 (K)	..	Ca
3968	H	
4101	*h*	
4130		
4172		
4227	..	Ca
4268		
4310	..	Hydrocarbon
4340	G	
4516		
4552		
4587		
4618		

For the eye observations, the new 3-foot mirror, which has recently been presented to the Astrophysical Laboratory by Mr. Common, was employed, but unfortunately the clock is not yet mounted, so that the observations were difficult.

C was the brightest line observed. In the green there were several lines, the brightest of which was in all probability F, the position being estimated by comparison with the flame of a wax taper. Another line was coincident, with the dispersion employed, with the radiation at λ 500 from burning magnesium wire. A fainter line between the two last named was probably near λ 495, thus completing the trio of lines which is characteristic of the spectra of nebulæ. There was also a fairly bright line or band coincident with the edge of the carbon fluting near λ 517 given by the flame of the

taper. A feeble line in the yellow was coincident, under the conditions employed, with the sodium line at D.

The hydrogen line at G was distinctly seen, as well as a band or group of lines between G and F.

Nearly all the lines appear to be approximately, if not actually, coincident with the lines seen in the various types of Cygnus stars, the chief difference being the apparent existence of carbon, hydrocarbon, and calcium in the nova.

The colour was estimated by Mr. Fowler as reddish-yellow, and by Mr. Baxandall as rather purplish. My own impression was that the star was reddish, with a purple tinge. This was in the 10-inch achromatic. In the 3-foot reflector it was certainly less red than many stars of Group II. No nebulosity was observed either in the 3-foot or the 10-inch refractor, nor does any appear in a photograph of the region taken by a $3\frac{1}{2}$-inch Dallmeyer lens with three hours' exposure. It should be stated that the camera was carried by the photographic telescope, the clock of which had had its normal rate purposely changed to give breadth to the spectrum.

The photographs were taken and reduced by Messrs. Fowler and Baxandall. The eye observations and comparisons were made by Mr. Fowler alone.

II. "Note on the Energy Absorbed by Friction in the Bores of Rifled Guns." By Captain NOBLE, C.B., F.R.S., &c. (late Royal Artillery). Received December 31, 1891.

The object of the experiments which I proceed to describe was to ascertain approximately, and under varied conditions, the loss of energy due to the friction of the driving ring of the projectile in the bores of rifled guns.

The rotation of modern breech-loading projectiles is generally given by means of a copper ring or band on the projectile, on a plan originally proposed by Mr. Vavasseur, the diameter of this ring being not only somewhat larger than that of the bore, but even larger than the diameter of the circle representing the bottom of the grooves, and the projections which give the rotation are formed by the pressure of the powder gases forcing the driving ring into the grooves of the gun. At the commencement of motion the driving ring is consequently exactly moulded to the section of the bore at the seat of the shot, and under the conditions due to the pressure to which the gun is at the moment subjected.

It will readily be conceived that a band or ring, moulded as described, may give rise to considerable friction in its passage through the bore, and the amount of this friction may be modified to a considerable extent by various circumstances.

For example, the nature of the powder employed may, depending on the deposit or fouling left in the bore, affect appreciably the friction. Again, the friction may be considerably modified by the form and diameter of the ring itself, while a variable amount of energy must be absorbed by the methods employed to give rotation, and by the amount of that rotation.

In the preliminary experiments three descriptions of powder were employed—(1) the powder known as P, or the pebble powder of the English Service; (2) an amide powder in which the nitrate of potassa of ordinary powder is largely replaced by nitrate of ammonia, and which powder, in addition to other valuable properties, gives rise to a smoke much less dense and much more rapidly dispersed than is the case with pebble and other similar powders; and (3) a true smokeless powder. The form of smokeless powder employed in this country is best known under the name of *cordite*, a propelling agent which promises to be of great value, and for which we are indebted to the labours and experiments of Sir F. Abel and Professor Dewar. A somewhat similar explosive is employed abroad under the name of "ballistite," and with this explosive also I have been able to make an interesting series of experiments. These experiments do not, however, come within the scope of the present note.

The preliminary experiments having shown that a very considerable amount of friction was, in the case of pebble powder, due to the fouling of the gun, while no such result was observed either in the case of the amide powder or the cordite, it was determined to carry out the subsequent experiments with the amide powder, firing, however, for purposes of corroboration an occasional round with the cordite, of which a small quantity only was available.

It may be of interest to note the loss of velocity and energy due to the fouling with pebble powder. The charge of powder in a 12-cm. gun being 12 lbs., and the weight of the shot 45 lbs., the velocity of the shot, the gun being carefully cleaned and oiled, was, in three trials, respectively, 1877 ft.-secs., 1877 ft.-secs., and 1878 ft.-secs. The two rounds fired immediately afterwards, the bore then being foul, were respectively 1850 and 1868 ft.-secs., 1848 and 1847 ft.-secs., 1852 and 1847 ft.-secs., or, taking the means of the whole series, the mean velocity with the gun clean was 1877·3 ft.-secs., with the bore foul 1852 ft.-secs., or, to put the result in another form, the mean energy realised from the pebble powder, the bore being carefully cleaned, and allowance being made for the energy of rotation, was 1102 ft.-tons, while the mean energy similarly realised with the bore foul was only 1072 ft.-tons, showing a loss of 30 ft.-tons or of 2·73 per cent. of energy attributable to the extra friction due to the powder deposit in the bore.

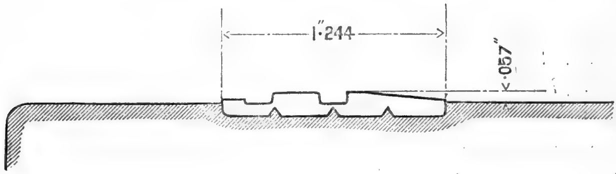
For the purposes of the subsequent experiments, three 12-cm.

quick-firing guns were specially prepared and rifled in the following manner:—The first had grooves of the usual section of the Service, but these grooves were all cut parallel to the axis of the bore, that is to say, the pitch of the rifling was infinite, or, in other words, there was no twist, and no rotation round the central axis would be communicated to the projectile; the second gun was rifled with a uniform pitch of 1 turn in 162 inches (about 1 turn in 35 calibres); while the third gun was rifled with a uniformly-increasing pitch of from 1 turn in 472″·5 at the breech to 1 turn in 162″ at the muzzle, so that in the last two guns, assuming the same muzzle velocity, the projectiles would leave the gun with the same angular velocity.

The projectiles used in these experiments were flat-headed cylinders (all being made of the exact weight of 45 lbs.), and differed from one another solely in the driving bands of the projectiles, which differed from one another both in diameter and length, the differences being shown in the sketches attached to the tabular results.

The first experiments were made with the rings marked "A," three rounds being fired from each of the three guns described, and the following table shows the velocities and energies obtained from each nature of gun.

Table I.—Results of Experiments with Driving Rings Section "A."



Nature of rifling.	Muzzle velocities.	Muzzle energies.	Mean muzzle velocity.	Mean muzzle energy.
	ft.-secs.	ft.-tons.	ft.-secs.	ft.-tons.
No twist	2130 2124 2136	1416 1408 1424	2130	1416
Uniform rifling	2109 2104 2118	1394 1386 1405	2110	1395
Parabolic rifling	2079 2088 2076	1354 1365 1350	2081	1356

Now, if the results given in this table be examined, it will be observed that the whole of the velocities obtained from the gun without twist are higher than those obtained from the gun rifled with a uniform twist, while the whole of the velocities obtained from the last-mentioned gun are higher than those obtained from the gun with the parabolic or uniformly increasing twist.

Using the mean results, there is a loss of velocity of 20 ft.-secs. in passing from the gun with no twist to that with a uniform twist, and a further loss of 29 ft.-secs., or 49 ft.-secs. in all, in passing to the gun with the parabolic rifling. Translating these losses of velocity into losses of energy, it appears that there is a loss of 21 ft.-tons, or about 1·5 per cent. of the total energy due to the uniform rifling, and a further loss of 39 ft.-tons, or 2·75 per cent., making 60 ft.-tons, or about $4\frac{1}{4}$ per cent., in all when the parabolic rifling is employed.

In a paper published in vol. 45 of the 'Philosophical Magazine' (1873) I investigated the ratio existing between the forces tending to produce translation and rotation in the bores of rifled guns, and I showed that, if R be the pressure tending to produce rotation, and G be the gaseous pressure acting on the base of the projectile, the resultant of which pressure acts along the axis of the bore, that is, along the axis of Z, then in the case of the parabolic rifling

$$R = \frac{2\rho^2(Gz + Mv^2)}{\dfrac{(h^2k^2 + 4\rho^2z^2)\sin\delta}{\sqrt{\{4z^2(\sin\delta)^2 + k^2\}}} + \dfrac{2\mu_1 kz(\rho^2 - h^2)}{\sqrt{(4z^2 + k^2)}}} \quad \ldots\ldots\ldots \quad (1),$$

where r is the radius of the bore, ρ the radius of gyration of the projectile, k the principal parameter of the parabola (the plane of xy being supposed to be at the vertex of the parabola and at right angles to the axis of the bore), δ the angle which the normal to the driving surface of the groove makes with the radius at the point under consideration, v the velocity at that point, μ_1 the coefficient of friction.

While in the case of a uniform twist

$$R = \frac{2\pi\rho^2 G}{\dfrac{\mu_1(2\pi\rho^2k - rh)}{\sqrt{(1+k^2)}} + \dfrac{(2\pi\rho^2 + rhk)\sin\delta}{\sqrt{\{k^2 + (\sin\delta)^2\}}}} \quad \ldots\ldots\ldots \quad (2),$$

where h is the pitch of the rifling, k the tangent of the angle which the groove makes with the plane of xy, the other constants, &c., bearing the meaning I have already assigned to them.

Now to obtain the numerical values of R from the above equations, a knowledge of the values of G, that is, of the total pressures acting on the base of the projectile, and in the case of the parabolic rifling of the velocity at all points of the bore, is necessary, and, the explosives

used being novel, for this investigation, as well as for other purposes, I have recently determined by direct experiments in the bore of a 12-cm. quick-firing gun the mean velocities and mean gaseous pressures at all points of the bore, both for the amide powder, mainly used in this investigation, and for cordite.

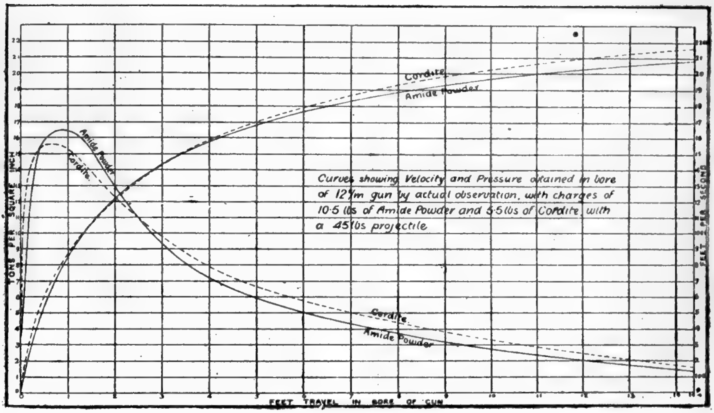


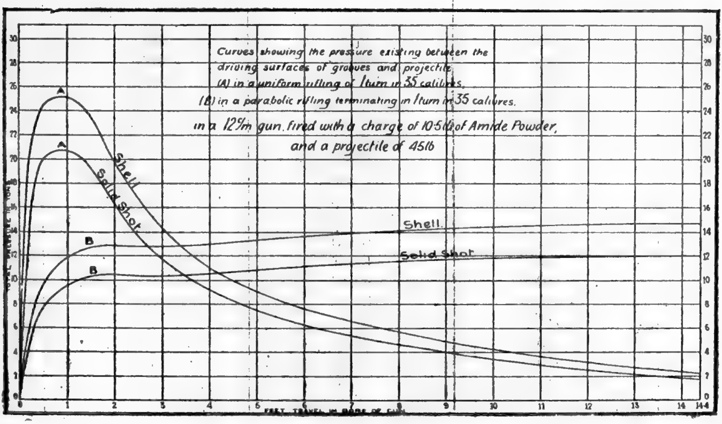
Table II.—Uniform Rifling, Amide Powder.

Travel of shot in bore in feet.	Total pressure, &c., on base of shot in tons.	Total pressure R between driving surface of grooves and ring of projectiles in tons.
0·5	254·7	19·9
1·0	264·0	20·7
1·5	245·0	19·2
2·0	207·9	16·3
2·5	175·7	13·7
3·0	150·7	11·8
4·0	115·2	9·1
5·0	94·9	7·4
6·0	80·6	6·3
7·0	69·5	5·4
8·0	60·0	4·7
9·0	52·1	4·1
10·0	44·8	3·5
11·0	38·4	3·0
12·0	32·9	2·6
13·0	28 4	2·2
14·0	24·3	1·9
14·4	22·6	1·8

The curve shown on p. 413 exhibits for the charges used and explosives I have named the results of these experiments, and, employing these values, the following tables give for uniform and parabolic rifling the value of R, that is, the pressure tending to give rotation calculated from formulæ (1) and (2). They also give the pressure acting on the base of the shot, and the velocity in the bore.

Table III.—Parabolic Rifling, Amide Powder.

Travel of shot in bore in feet.	Total pressure on base of shot in tons.	Velocity, ft.-secs.	Total pressure R between driving surface of groove and ring of projectile in tons.
0·5	254·7	548	7·9
1·0	264·0	849	9·7
1·5	245·0	1064	10·3
2·0	207·9	1224	10·5
2·5	175·7	1343	10·5
3·0	150·7	1437	10·4
4·0	115·2	1577	10·5
5·0	94·9	1680	10·8
6·0	80·6	1761	11·1
7·0	69·5	1828	11·4
8·0	60·0	1884	11·6
9·0	52·1	1931	11·8
10·0	44·8	1970	11·9
11·0	38·4	2004	12·0
12·0	32·9	2032	12·0
13·0	28·4	2056	12·1
14·0	24·3	2076	12·1
14·4	22·6	2084	12·1

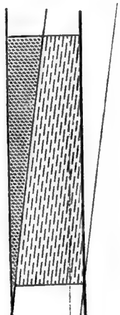


The values of R as given in the last columns of the above tables are graphically shown on p. 414, and from a comparison of the two curves it will be readily seen that, although the maximum pressure between the driving surfaces is not so high with the parabolic as with the uniform rifling, yet, as has been pointed out by Professor Osborne Reynolds, the mean driving pressure is with the parabolic rifling considerably higher, and as the energy absorbed by the friction between the driving surfaces is approximately proportional to the mean driving pressures, the loss of energy with that form of rifling is appreciably greater than with the uniform rifling.

In the experiments I am now discussing the mean driving pressure throughout the bore was, with the uniform rifling, 7·35 tons; the mean loss of energy due to the uniform rifling was 21 ft.-tons; hence the coefficient of the friction between the driving surfaces derived from these particular experiments is $\mu = 0{\cdot}199$.

Again, with the parabolic rifling, the mean driving pressure throughout the bore is 11·06 tons, and if we had only a similar friction to consider, the loss of energy with this rifling should be proportioned to the pressure. The loss, however, is much higher, amounting, in fact, to 60 ft.-tons. Part of this extra loss must be ascribed to the continual alteration of form that the copper driving ring is subjected to in its passage up the bore,* but it seems to be doubtful if the whole of this loss can be ascribed to this cause. Part may possibly be ascribed to the ribs being continually forced, so to speak, to ride on to the sloping driving surface; but the number of rounds in each case being few, a part may possibly be ascribed to

* The action I refer to will readily be understood from the annexed diagram. If the thick lines represent the plan of one of the grooves at the initial angle of the rifling, the projections on the driving ring will be moulded into that form, and if the light lines represent the groove at its terminal angle it will be seen that the final form of the projections on the ring will be as shown by the shading, while the cross-hatched portion represents the metal removed by the action of the driving surface.



variations in the energy developed in the gun. Variations in energy, under precisely similar conditions, might easily amount to 1 or 2 per cent., or occasionally more, and, as will be subsequently seen, the differences between the uniform and parabolic rifling, although always in the same direction, are not the same in all the series, and the mean of the whole will probably give the most reliable result.

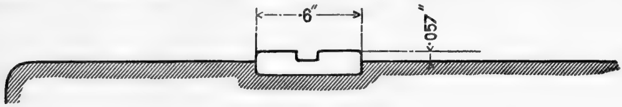
Summing up the results at which we have so far arrived in the experiments I have discussed, it appears that the total loss of energy arising from the fouling of pebble powder and from the friction due to the parabolic rifling together amounted to close upon 7 per cent. of the whole energy developed.

The third and subsequent series of experiments were made some weeks later, and from climatic or other causes there was a slight but decided decrement in the energy obtained with the amide powder. This decrement did not in any way affect the experiments except that the absolute values of the energies at the different dates are not strictly comparable.

The object of the third series was to ascertain if a narrow driving band would rotate the projectile equally well, as with an increasing twist it is important, if rotation be secured, that the breadth of the driving band be as small as is convenient, and further, as in the last series, to ascertain the loss of energy due to the uniform and parabolic rifling.

The results of this third series were as shown in the following table:—

Table IV.—Results of Experiments with Rings of Section "B."



Nature of rifling.	Muzzle velocity.	Muzzle energies.	Mean muzzle velocities.	Mean muzzle energies.
	ft.-secs.	ft.-tons.	ft.-secs.	ft.-tons.
No twist	2112 2104 2124	1392 1381 1408	2113	1394
Uniform twist	2109 2094 2095	1393 1373 1375	2099	1380
Parabolic twist.......	2067 2066 2066	1338 1337 1337	2066	1337

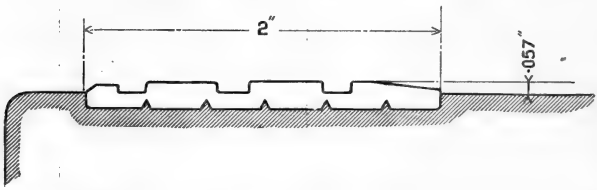
The results of this series confirm generally those of the previous series. The loss of energy due to the friction of the uniform rifling amounts to 14 ft.-tons, or, a little more than 1 per cent., while that due to friction and other causes with the parabolic rifling amounts to 57 ft.-tons or about 4·1 per cent., and nearly the same as before. The difference between the uniform and parabolic rifling should have been less than in the former series; as a matter of fact it is greater, but this may be accounted for by variations in the powder as previously suggested, as the suppression of a single round in each of the two guns would make the results in accordance with theory.

The coefficient of friction calculated from the uniform rifling gives $\mu_1 = 0{\cdot}133$.

The driving ring in this series was amply sufficient for rotative purposes, there not being even with the highest velocity obtained the slightest appearance of slip or undue wear.

In the fourth series the driving ring was of the Government pattern, but longer, and as is shown in section "C," and the results obtained were as given in the table.

Table V.—Results of Experiments with Driving Rings of Section "C."



Nature of rifling.	Muzzle velocities.	Muzzle energies.	Mean muzzle velocities.	Mean muzzle energies.
	ft.-secs.	ft.-tons.	ft.-secs.	ft.-tons.
No twist	2111 2114 2114	1417 1394 1394	2120	1402
Uniform twist	2092 2082 2088	1371 1358 1365	2087	1364
Parabolic rifling	2068 2066 2071	1339 1337 1343	2068	1340

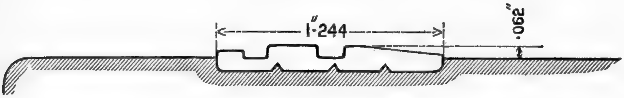
The loss of velocity due to the uniform and parabolic rifling is, from these experiments, respectively 33 and 52 ft.-secs., and the loss

of energy respectively 38 and 62 ft.-tons, or, expressed in percentages, 2·71 per cent. for the uniform rifling and 4·72 per cent. (the highest reached) for the parabolic rifling.

The value of μ_1, the coefficient of friction, calculated from the uniform rifling, is 0·359.

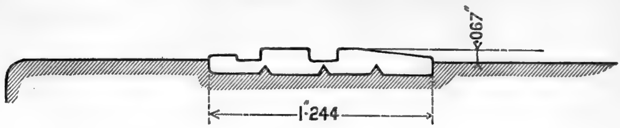
The fifth and sixth series were fired with driving bands of the Government pattern, but with radii successively slightly increased, as shown in the diagrams, and the results are given in the two following tables.

Table VI.—Results of Experiments with Driving Rings of Section "E."



Nature of rifling.	Muzzle velocities.	Muzzle energies.	Mean muzzle velocities.	Mean muzzle energies.
	ft.-secs.	ft.-tons.	ft.-secs.	ft.-tons.
No twist............	2132 2124 2123	1418 1408 1406	2126	1411
Uniform rifling.......	2113 2115 2114	1398 1401 1399	2114	1399
Parabolic rifling	2099 2095 2081	1380 1375 1356	2092	1370

Table VII.—Result of Experiment with Driving Rings of Section "F."



Nature of rifling.	Muzzle velocities.	Muzzle energies.	Mean muzzle velocity.	Mean muzzle energy.
	ft.-secs.	ft.-tons.	ft.-secs.	ft.-tons.
No twist............	2112 2141 2141	1392 1430 1430	2131	1417
Uniform rifling.......	2104 2110 2124	1378 1384 1413	2113	1395
Parabolic rifling......	2093 2099 2094	1372 1380 1373	2095	1375

From these two tables it will be seen that the loss of velocity due to the uniform and parabolic rifling is, in Table VI, 12 ft.-secs. and 64 ft.-secs. respectively; and in Table VII, 18 ft.-secs. and 36 ft.-secs. respectively; these velocities corresponding to losses of energy of 12 ft.-tons and 22 ft.-tons due to the uniform twist, and 41 ft.-tons and 42 ft.-tons, or about 3 per cent., due to the parabolic rifling. Calculated as before from the uniform rifling, the coefficients of friction are respectively 0·114 and 0·208.

Examining now with respect to the uniform rifling the whole of the series I have described, and observing that with this rifling the particular form or width of the driving ring would have but a very slight, if any, effect upon the loss of energy due to friction, it will be seen, from Table VIII, that the mean loss of energy amounts to 1·52 per cent. of the total energy corresponding to a mean coefficient of friction of 0·203, or, say, 0·2.

If, as I have pointed out, the loss of energy in the parabolic rifling was proportional to the pressure on the driving surfaces, the additional loss due to that rifling would be 0·74 per cent. The actual additional loss is, on the mean of the whole of the experiments, about three times as great, the mean loss due to parabolic rifling being, as shown by Table VIII, 3·78 per cent., and this considerable increment may be ascribed to the causes I have mentioned.

Table VIII.—Showing the percentage of Loss of Energy due to Friction in the various Series; showing also the Deduced Value of the Coefficient of Friction.

Series.	Loss due to uniform rifling.	Loss due to parabolic rifling.	Coefficient of friction.
	per cent.	per cent.	μ_1.
2	1·48	4·23	0·199
3	1·01	4·09	0·133
4	2·71	4·72	0·359
5	0·85	2·90	0·114
6	1·55	2·97	0·208
Means ..	1·52	3·78	0·203

It may be worth while to mention that, in the groove formerly used in the Service, the angle between the normal to the driving surface and the radius could, without serious error, be taken as $= 90°$. In the groove adopted in the guns under consideration the mean value of δ is only about 34° 45′, and this difference in the driving angle ncreases the value of R, and, in consequence, the friction, by about 76 per cent. It would be interesting to make careful experiments to ascertain if there be any measurable difference in energy if an angle more nearly approaching to 90° were adopted. On account of the different length of the radius of gyration in the case of a solid shot and of a shell, the value of R is considerably affected when the latter projectile is fired. The difference of values is shown by the curves on p. 414.

In nearly all the countries of Europe an increasing twist is the form of rifling usually adopted; and, with such a consensus of practice, it must be assumed that some advantage is supposed to be gained by its use. There is, of course, with the parabolic rifling a less maximum pressure on the driving surfaces; but, as far as energy is concerned, both theory and the experiments I have detailed concur in showing that there is a distinct and very appreciable loss resulting from its employment. It is quite possible, although I am not acquainted with any carefully-conducted experiments on the point, that superior accuracy may be the advantage obtained; and if this were decidedly so, a loss of one or two per cent. of energy would not be, perhaps, a serious price to pay; but as, without any inconvenience, the question of accuracy could be easily settled, I trust that before very long this point also may be definitely determined.

It only remains to give the results obtained with cordite. At the time the experiments were made, I had only at my disposal a very limited amount of this explosive, and I was only able to fire one

round in each of the guns, using the driving rings marked A, B, and C. As it would be useless to attempt to draw general conclusions from single rounds, and as in guns of the calibre experimented with the difference between the driving rings is not very marked, I have treated the series as if all the rounds had been fired with the same driving ring; the results are given in Table IX.

Table IX.—Results of Experiments with Cordite.

Nature of rifling.	Muzzle velocities.	Muzzle energies.	Mean muzzle velocities.	Mean muzzle energies.
	ft.-secs.	ft.-tons.	ft.-secs.	ft.-tons.
No twist	2177 2171 2194	1479 1476 1509	2181	1488
Uniform rifling	2160 2161 2172	1461 1462 1477	2164	1467
Parabolic rifling.......	2156 2152 2157	1455 1450 1457	2155	1454

From the cordite experiments, it follows that the loss of energy due to the uniform rifling is 21 ft.-tons, or 1·43 per cent., and to the parabolic rifling 34 ft.-tons, or 2·3 per cent.: the coefficient of friction deduced from the loss of energy with the uniform rifling being 0·199, or nearly the same value as was given in Table VIII.

III. "On the Thermal Conductivities of Crystals and other Bad Conductors." By CHARLES H. LEES, M.Sc., late Bishop Berkeley Fellow at the Owens College, Manchester. Communicated by Professor ARTHUR SCHUSTER, F.R.S. Received January 22, 1892.

(Abstract.)

The author commences by pointing out the great differences between the results obtained in 1879 by G. Forbes for the conductivities of quartz in different directions and those obtained in 1883 by Tuschmidt. He then refers to Kundt's discovery, that the metals stand in the same order as conductors, and as to the velocity of propagation of light through them, and mentions that his

experiments were originally intended to furnish data for a similar comparison for crystals, but that their object has been extended.

After some preliminary experiments, he adopted the "divided bar" method, which consists in placing a disc of the material the conductivity of which is required, between the ends of two bars of metal placed coaxially, heating one end of the combination, and observing, by means of thermo-junctions applied to the bars, the distribution of temperature along them, first, with the disc in position, second, with the bars in contact without the disc. When the conductivity of the bar is known, these observations suffice to determine that of the disc.

The bars used were 1·9 cm. diameter, and about 34 cm. long. The ends which came in contact with the discs were amalgamated, as this was found to be the best method of securing good contacts. These bars were suspended horizontally in a frame, by means of strings passing over adjusting screws, which enabled the bars to be set accurately in the required position. The temperatures were found by means of a copper-platinum-silver junction applied to points along the bars, at which small conical holes about 0·5 mm. diameter, containing mercury, were placed. This junction was in circuit with a galvanometer, and the circuit was so arranged that its resistance could be found by a modification of Thomson's bridge method.

The conductivity of the brass bar was determined before cutting, by the method—due to Forbes—of determining the loss of heat from the surface by allowing the bar to cool and observing the change of temperature with time, and then observing the steady distribution of temperature along the bar when heated at one end.

The author shows that change of both the "internal" and "external" conductivities with temperature must be taken into account in the equation for the distribution of temperature. He takes each to be a linear function of the temperature, and finds finally the conductivity of the bar to be 0·27 c.g.s. unit, and to increase slightly with the temperature.

The discs used were of the same diameter as the bar, and were of various thicknesses, in order to make the distribution of temperature throughout the bars nearly the same in each case.

The following are the results obtained, the conductivities of a few other bodies being given, in order to show the positions of the bodies experimented on amongst conductors generally. No relation of the kind found by Kundt for metals seems to hold for the crystals experimented on:—

	c.g.s. units.	
Copper	—	0·7 to 0·8 (Lorenz, &c.).
Brass	0·27	0·25 to 0·3 ,,
Bismuth	—	0·017 ,,
Mercury	—	0·018 Ångström.
Crown glass	0·0024	0·0016 (H. Meyer).
Flint glass	0·0020	0·0014 ,,
Glass	—	0·0021 (Peclet). 0·0005 (G. Forbes).
Rock salt	0·014	0·016 (Tuschmidt).
Quartz along axis	0·030	0·026 (Tuschmidt). 0·001 (G. Forbes).
,, perpendicular to axis	0·016	0·004 ,, 0·016 (Tuschmidt).
Iceland spar along axis	0·010	0·016 ,,
,, perpendicular to axis	0·0084	0·008 ,,
Mica perpendicular to cleavage	0·0016	
White marble	0·0071	0·007 (Peclet). 0·001 (G. Forbes).
Slate	0·0047	0·0008 ,,
Water	—	0·0015 (Winkelmann).
Glycerine	—	0·0007 ,,
Olive oil	—	0·0004 (G. Weber).
Shellac	0·00060	
Paraffin	0·00061	0·0001 (G. Forbes).
Pure rubber	0·00038	0·00009 ,, 0·0005 (Peclet).
Sulphur	0·00045	
Ebonite	0·00040	0·00008 (G. Forbes).
Gutta percha	0·00046	
Paper	0·00031	
Asbestos paper	0·0006	
Mahogany	0·00047	
Walnut	0·00036	
Cork	0·00013	
Silk	0·00022	
Cotton	0·00055	
Flannel	0·00023	

IV. "On the Mechanical Stretching of Liquids: an Experimental Determination of the Volume-Extensibility of Ethyl Alcohol." By A. M. WORTHINGTON, M.A. Communicated by Professor POYNTING, F.R.S. Received February 1, 1892.

(Abstract.)

After adverting to the three known methods of subjecting a liquid to tension, viz., (i) the method of the inverted barometer, (ii) the

centrifugal method devised by Osborne Reynolds, (iii) the method of cooling discovered in 1850 by Berthelot, and pointing out that the first two afford means of measuring stress but not strain, while the third gives a measure of strain but not stress, the author proceeds to describe the manner in which he had used the method of Berthelot in combination with a new mode of determining the stress, and had succeeded in obtaining simultaneous measures of tensile stress and strain for ethyl alcohol up to a tension of more than 17 atmospheres, or 255 lbs. per square inch.

The liquid, deprived of air by prolonged boiling, is sealed in a strong glass vessel, which it almost fills at a particular temperature, the residual space being occupied only by vapour. On raising the temperature, the liquid expands and fills the whole. On now lowering the temperature, the liquid is prevented from contracting by its adhesion to the walls of the vessels, and remains distended, still filling the whole and exerting an inward pull on the walls of the vessel. The tension exerted is measured by means of the change in capacity of the ellipsoidal bulb of a thermometer sealed into the vessel and called the "tonometer." This bulb becomes slightly more spherical, and therefore more capacious, under the pull of the liquid, and the mercury in the tonometer stem falls. The tension corresponding to the fall is previously determined from observation of the rise produced by an equal pressure applied over the same surface.

The liquid is caused at any desired instant to let go its hold and spring back to the unstretched volume corresponding to its temperature and to its saturated vapour-pressure by heating for a moment, by means of an electric current, a fine platinum wire passing transversely through the capillary tube that forms part of the vessel. The space left vacant in the tube represents the *apparent* extension uncorrected for the yielding of the glass vessel.

The measures obtained show that, within the limits of observational error, the stress and this appparent strain are proportional up to the highest tension reached (17 atmospheres); but, since the small yielding of the nearly rigid glass vessel must itself be proportional to the stress, it follows that the stress and absolute strain are proportional.

By subjecting the liquid to a pressure of 12 atmospheres *in the same vessel*, it was found that the apparent compressibility was the same as the apparent extensibility, whence it is deduced that between pressures of +12 and −17 atmospheres the absolute coefficient of elasticity is, within the limits of observational error, constant. Its actual value is best obtained by observations of compressibility.

The paper concludes with a description and explanation of a peculiar phenomenon of adhesion between two solids in contact when immersed in a liquid that is subjected to tension.

Presents, February 4, 1892.

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Nine *Carte de Visite* Photographs of Fellows of the Royal Society.
Messrs. Maull and Fox.

February 11, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

The Chairman read the following Letter:—

Whitehall,
5th February, 1892.

SIR,

I have had the honour to lay before the Queen the loyal and dutiful Address of the Fellows of the Royal Society of London on the occasion of the death of His Royal Highness The Duke of Clarence and Avondale, K.G., and I have to inform you that Her Majesty was pleased to receive the Address very graciously.

I have the honour to be,
Sir,
Your obedient Servant,
HENRY MATTHEWS.

The Treasurer and Vice-President of the
Royal Society of London.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Note on the Spectrum of Nova Aurigæ." By J. NORMAN LOCKYER, F.R.S. Received February 8, 1892.

Since the observations of Wednesday (Feb. 3), recorded in a preliminary note, the weather precluded any further work till last night (Feb. 7). Two more photographs were taken and eye observations made.

The photographs, though exposed for a shorter time, gave many more lines than the long-exposed one on Wednesday.

The bright lines at K, H, *h*, and G are accompanied by dark lines on their more refrangible sides.

Addendum. Received February 11.

Eye Observations.

On account of continued bad weather, no further photographs or observations of the Nova have been obtained since February 7. It

then appeared to be slightly brighter than on February 3, when the star was first observed at Kensington. With the 10-inch refractor and Maclean spectroscope, C was seen to be very brilliant, and there were four very conspicuous lines in the green. Several fainter lines were also seen, and a dark line was suspected in the orange. I noticed that some of the lines, especially the bright one near F, on the less refrangible side, appeared to change rapidly in relative brightness, and this was confirmed by Mr. Fowler.

Observations of the spectrum were made by Mr. Fowler with the 3-foot reflector and the Hilger 3-prism spectroscope. Of the four most conspicuous lines in the green, F is the most refrangible, and comparisons with burning magnesium showed one of them to be sensibly coincident with the edge of the magnesium fluting at 500·6. The least refrangible of the four bright green lines was found to be slightly less refrangible than the carbon fluting near λ 517; it gives no indications of a fluted character, and further observations seemed to suggest that it was magnesium *b*, unless there be a very great change of position due to motion in the line of sight. The fourth line, which lies between F and 500·6, is about one-third of the distance between them from F, and its wave-length, assuming the star to be at rest, was estimated to be about 490.

In addition to these, the G line of hydrogen was distinctly visible, and also a group of lines between G and F. The latter were not measured, as they appear on the photographs.

Amongst the fainter lines, one was estimated to be near λ 527, and is probably the iron line at E. By comparison with the spectrum of manganese chloride burning in a spirit-lamp flame, another line was found to be sensibly coincident with the edge of the brightest fluting, λ 557·6.

There was a bright line a little more refrangible than C, and the D line was faintly visible.

Photographs.

The first photograph was exposed from 10.20 to 11.50 P.M., and the second from 12 to 2 A.M., Feb. 7, the 6-inch object-glass and prism being employed in each case. The same number of lines is shown in both photographs, the sky not being so clear during the second as during the first exposure. Twenty bright lines have been measured, and their wave-lengths are given in the accompanying table.

The table also shows probable coincidences with the lines in the spectra of the Wolf-Rayet stars, as photographed by Professor Pickering; dark lines in the Orion stars, photographed at Kensington; and bright lines in the Orion nebula, photographed at Westgate. This part of the subject will be discussed in a subsequent paper.

Lines in the spectrum of Nova Aurigæ.				Bright-line stars.	Orion stars (dark lines).	Nebula in Orion (bright lines).
1st photo.	2nd photo.	3rd photo.	3rd photo.			
Date, Feb. 3.	Date, Feb. 3.	Date, Feb. 7.	By direct comparison with α Cygni.			
By curve.	By curve.	By curve.				
K 3933	3933	3933	3933	—	3933	3933
H 3968	3968	3968	3968	3970	3968	3968
h 4101	4101	4101	4101	4101	4101	4101
4128	4130	4127	4128	—	4130	4130
4172	4172	4172	4172	—	4172	—
—	—	—	4202	4200	—	4200
4226	4227	4228	4226	—	—	42 6
4268	4268	—	4264	—	4268	4268
—	—	4294	4291	—	—	—
4312	4310	4310	4310	—	—	—
G 4340	4340	4340	4340	4340	4340	4340
—	—	—	4383	—	—	4383
—	—	—	4412	—	4415	4410
—	—	—	4434	—	—	—
—	—	—	4469	4472	4472	4472
4516	4516	4522	4518	4510	—	—
4552	4552	4554	4555	4550	—	—
4587	4587	4584	4587	—	—	—
4618	4618	4625	4625	4620	—	—
—	—	4860	F 4860	4860	4860	4860

In addition to the lines recorded in the table, the photographs of the spectrum of the Nova showed several lines more refrangible than K. These have not yet been reduced, but they probably include some of the ultra-violet hydrogen lines.

All the lines in the spectrum of the Nova are broad, although in a photograph of the spectrum of Arcturus, taken with the same instrumental conditions, the lines are perfectly sharp. It is important to note that the broadening of the lines is not accompanied by any falling off of intensity at the edges, as in the case of the hydrogen lines in such a star as Sirius. With the method employed in taking the photographs, long exposures are liable to result in a thickening of all the lines, on account of atmospheric tremors. The lines would also be thick if the Nova be hazy, as observed at Greenwich. In the photographs, however, all the lines are not equally thick.

If the lines are similarly broadened when a slit spectroscope is employed, the effect must be due to internal agitations; for if different regions of the Nova are moving with varying velocity, or with the same velocity in different directions, a normally fine line might be widened, as observed in the photographs.

The hydrogen lines and the K line of calcium are very bright, and, as pointed out in the note above, they are accompanied by dark lines on their more refrangible sides. This was previously noticed in the photographs taken on February 3, but as the dark lines were not very conspicuous, they were not referred to until further confirmation had been obtained.

It appears from a note in the 'Standard' newspaper, February 10, that dark lines have also been observed on the more refrangible sides of the bright hydrogen lines in the photographs taken at Harvard College Observatory.

A somewhat similar phenomenon has already been recorded by Professor Pickering, in the case of β Lyræ, and this has been confirmed by a series of photographs taken at Kensington. In this case, the bright lines are alternately more and less refrangible than the dark ones, with a period probably corresponding to the known period of variation in the light of the star. The maximum relative velocity indicated is stated by Professor Pickering as approximately 300 English miles per second.

In the case of Nova Aurigæ, the dark lines in all four photographs taken at Kensington are more refrangible than the bright ones, so that as yet there is no evidence of revolution. The relative velocity indicated by the displacement of the dark lines with respect to the bright ones appears to be over, rather than under, 500 miles per second. The reduction is not yet complete.

Should the photographs which may be obtained in the future continue to show the dark lines displaced to the more refrangible side of

the bright ones, it will be a valuable confirmation of my hypothesis as to the causes which produce a new star, namely, the collision of two meteor swarms. On this supposition, the spectrum of Nova Aurigæ would suggest that a dense swarm is moving towards the earth with a great velocity, and passing through a sparser swarm, which is receding. The great agitation set up in the dense swarm would produce the dark line spectrum, while the sparser swarm would give the bright lines.

In taking the first photograph, I was assisted by Mr. Fowler; the second was taken by Messrs. Fowler and Shackleton. Mr. Baxandall is responsible for the determination of the wave-lengths of the lines, and Mr. Shackleton for the determination of relative velocity.

II. "Contributions to the Physiology and Pathology of the Mammalian Heart." (From the Cambridge Pathological Laboratory.) By C. S. Roy, M.D., F.R.S., Professor of Pathology, and J. G. Adami, M.A., M.B., Fellow of Jesus College, Cambridge. Received December 31, 1891.

(Abstract.)

Our communication begins by stating that we have sought to study the action of the Mammalian heart in conditions (unexcised and intact) as nearly approaching the normal as we were able to make compatible with the employment of exact methods of research. This is followed by a general consideration of the difficulties attendant upon such a study, and of the means by which these difficulties may be overcome.

Under the heading of Methods we describe a *cardiometer* which we employed to measure the contraction volume and the "output," as well as the changes in the volume of the heart other than those due to its rhythmic contractions and expansions. A description is also given of the method of employing it, together with a statement as to the degree of the accuracy with which, according to our experience, the instrument supplies information regarding the changes in the volume of the heart. We then describe an automatic counter, which we employed for measuring out and recording the output of the heart, as obtained by the cardiometer.

This is followed by a description of our *myocardiograph*, which we made use of to record the contractions and expansions of any part or parts of the ventricular and auricular walls without interfering with the movements of the heart. In most cases we employed this instrument to obtain simultaneous records of the contractions of one auricle and one ventricle. We state also our doubts as to the

value of observations made on the heart by "button" cardiographs.

Section III begins by a consideration of the relationship between the circumference of a hollow spherical muscle and its cubic contents, this being illustrated by a diagram, and by one or two concrete examples with regard to the bearing of this subject upon the physiology of the ventricles.

We then state the relation between the internal circumference of a hollow spherical muscle and the resistance to contraction of its walls. Reference is also made to the elastic resistance which the heart wall itself offers to contraction, and the bearing of this upon the production of negative pressure within its cavity under certain conditions.

We then consider briefly the effect on the ventricular contractions of changes in the blood pressure within the systemic and pulmonary arteries, pointing out how much the heart has in common with the voluntary muscles of the body, and explaining why the amount of residual blood is liable to changes, concluding with a few remarks upon "failure of the heart."

In Section IV we enter upon a study of the effects of the vagus nerve upon the heart. We begin with the changes in the contraction volume, and point out that, at first sight, our curves seem to show that, other things being equal, the volume of blood expelled at each systole varies in inverse ratio to the rapidity of heart beat. We show, however, that this general law does not hold good for vagus slowing (if, indeed, it be exact for slowing of any kind), which is found to be accompanied by a lowering of the output; that, with moderate slowing, this diminution of the output may be as much as 30 or 35 per cent.

We then speak of the increase in the amount of residual blood in the heart which is produced by vagus excitation, showing that this does not necessarily indicate any weakening of the ventricular contractions.

We next analyse myocardiographic records of the action of the vagus upon the heart, showing that the auricular contractions are weakened or arrested, and noting that the influence of the vagus upon the force of the auricular contractions bears no constant proportion to the vagus slowing. By strong vagus excitation or by muscarin the auricles may be completely arrested, it may be, for hours. This complete arrest is, in some cases, led up to by progressive weakening, but sometimes arrest occurs immediately after fairly strong beats, or with fairly strong beats presenting themselves at times during the arrest. These latter cases may be explained by weakening of the excitations which reach the auricles from the sinus, although they are possibly due to diminished excitability of the auricles.

On coming to the effect of the vagi upon the ventricles we find that the distension of the heart during vagus actions is due to the ventricles being more expanded, both in diastole and in systole. We point out that the increased volume of the heart at the end of systole is a necessary result of the increased contraction volume, and combat the conclusions of those who ascribe it to weakening of the ventricular contractions, pointing out that the greatly increased contraction volume increases to a corresponding extent the work done at each contraction. We give detailed reasons for concluding that this suffices to explain the apparent diminution of the ventricular contractions.

We then examine the influence of the vagus upon the tonus of the relaxed ventricles, and point out that the great distension during vagus action is due entirely to increased intra-ventricular pressure during diastole, and not, as has been asserted by some, to any change in the elasticity of the relaxed ventricular wall.

Next, we consider the cause of the rise of venous (systemic and pulmonary) pressure, and find that this is due not to any increase in the amount of blood entering the veins in a given time or to contraction of their walls, but that it is to be ascribed to the diminished inflow into the ventricles.

The cause of this diminished inflow into the ventricles leading to corresponding diminution of the output is twofold, namely, weakening or arrest of the auricles, and, secondly, the elastic resistance of the ventricular wall to distension. We show that this explanation must apply to both sides of the heart, and that observed facts correspond with it.

We then consider the after-effects of vagus excitation, and show that the temporary increase in the output which is sometimes present may be explained by a temporary increase in the force of the auricular contractions, and by the venous pressure taking some little time to fall after the vagus excitation has ceased.

After this, we examine the influence of the vagus upon the heart rhythm, and show that, when the vagus excitation reaches a certain degree (varying in different animals), the ventricles begin to beat independently of the sinus and auricles; that this rhythm, which is at first slow and irregular, gradually becomes fairly rapid and almost completely regular.

This rhythm, we show, must be looked upon as the same as that which, as Wooldridge and Tigerstedt observed, makes its appearance when the ventricles are severed from the auricles. We point out, however, that the independent ventricular rhythm of vagus action is characterised by the slowness with which it establishes itself.

This characteristic is due to the lowering of the excitability of the ventricles produced by vagus action, and we adduce a considerable

number of facts showing that the vagus *does* lower the excitability of the ventricles, and that, by means of muscarin and by discontinuous stimulation of the vagus, it is possible to isolate the influence of the vagus on the rhythm and force of the auricles from its influence upon the excitability of the ventricles. The power of the vagus to stop the ventricles temporarily can only be explained by this diminution of their excitability.

We show that, with a certain degree of vagus excitation, irregularity of the ventricles necessarily results, in consequence of the sinus and the ideo-ventricular rhythms interfering with one another; that this is the common cause of irregularity; and that irregularity may also be caused by the auricles not responding to all the impulses which reach them from the sinus.

We explain that, in rare instances, direct excitation of the vagus may so lower the excitability of the ventricle that the contractions may not extend over the whole of their walls, and may in this way produce the apparent weakening which is sometimes met with.

In Section V we pass on to study the effect of direct excitation of the *nervi augmentores* (*accelerantes*) upon the heart, and show that the acceleration of the rhythm may be extremely slight if the heart be beating fast, and that the acceleration and augmentation of force of the heart bear no constant proportion to one another. The augmentor nerves increase the diastolic expansion of the auricles and also increase their systolic contraction; but these two effects do not go hand in hand.

Excitation of the augmentors increases the output of the heart, owing to the increased force and frequence of the auricular contractions, the result of this being that the pressures in the systemic and pulmonary arteries rise, while the systemic and pulmonary venous pressures fall. If there be but little quickening, the contraction volume of the ventricles is increased.

The augmentors, on direct stimulation, cause a slight increase in the diastolic expansion of the ventricles, which is passive in nature and due to the increased force of the auricular contraction. The force of the ventricular contractions is increased; they contract more completely, diminishing the amount of residual blood, in spite of the fact that the arterial pressure is usually somewhat raised.

There are certain nerve fibres other than the *nervi augmentores* proper which pass from the stellate ganglion to the heart, sometimes by the annulus of Vieussens to the inferior cervical ganglion, but sometimes as separate branches passing directly to the heart from the ganglion stellatum, or the annulus. On peripheral excitation of the cut nerves there is marked weakening of the contractions both of the auricles and of the ventricles, usually with some degree of slowing, this being sometimes followed on cessation of the excita-

tion by a very well-marked increase in the force and frequence of the auricular and ventricular contractions. They may be vaso-constrictors for the coronary vessels, although we give no proof of this.

There are nerve fibres which descend to the heart by the vago-sympathetics, which, on excitation under certain conditions, increase the force and frequence of beat of the auricles and ventricles, and which may be vaso-dilators for the coronary vessels.

Reflex excitation of the vagus produces results which are the same as those of direct excitation of the nerve, and the curves are more typical and satisfactory than those obtained on direct excitation of the nerve.

Excitation of a mixed nerve like the sciatic usually produces effects on the heart similar in kind to those due to direct excitation of the augmentors, but the phenomena are complicated by the greater rise of the pressure in the systemic arteries. Sometimes the increase in force of the ventricle more than counterbalances this increased resistance to contraction, and the amount of residual blood in the left ventricle is reduced; in other cases the increase in force of the ventricular contractions is not sufficient to counterbalance the increased resistance, and the residual blood in the left ventricle is increased.

In Section IX we show that excitation of the central end of a mixed nerve like the sciatic or splanchnic usually affects both the augmentor and vagus centres in the medulla, and that, in nearly all cases, the augmentor centre is the more strongly excited of the two, so that augmentor effects show themselves during the excitation, but are succeeded by vagus action on ceasing to excite the nerve. In many cases augmentor effects alone show themselves. When excited reflexly the augmentor centre ceases to act earlier than the vagus; the opposite, therefore, to what takes place with direct excitation. In rare cases the excitation of the vagus centre may be stronger than that of the augmentor from the first. Although, in the absence of any augmentor action, the vagus does not reduce the force of the ventricular systole, it does unmistakably have the power of inhibiting the strengthening influence which the augmentors exert upon the ventricular contractions.

In Section X, upon the part played by the vagus in the economy, we show that vagus excitation relieves the heart of work and therefore of waste to as great an extent as is compatible with a continuation of the circulation, and conclude that the vagus acts as a protective nerve to the heart, reducing the work thrown upon that organ when from fatigue or other cause such relief is required by it. The presence of fibres in the sciatic and other mixed nerves which cause reflex excitation of the vagus would seem to indicate that this nerve may be used by other parts of the body to diminish the out-

put of the heart and lower the blood pressure, thereby reducing the activity of the circulation as a whole. The influence of the blood pressure in the systemic arteries on the degree of vagus activity and the readiness with which the vagus centre is called into play by raising the intercranial pressure indicate that the vagus mechanism is specially employed in lowering the circulation so as to limit cerebral congestion. The vagus acts chiefly in the interests of the heart and central nervous system.

The power of the vagus over the heart is limited, and the ideo-ventricular mechanism, which comes into play when the vagus action exceeds a certain limit, must be looked upon as the means by which arrest of the circulation and death is prevented, whenever from any cause the nerve exerts a maximum influence. The power of the vagus to lower the excitability of the ventricles makes their temporary arrest possible, but this reduction of the excitability of the ventricles cannot be kept up, no matter how strong the stimuli applied to the nerve, for a period long enough to endanger the economy.

In Section XI we show that the function of the augmentor in the economy is to increase the work and tissue waste of the heart as part of the mechanism by which the nervous system governs the circulation, and that the augmentor mechanism sacrifices the heart in order to increase the output of the organ and enable the ventricles to pump out their contents against a heightened arterial pressure. Such excessive action of the heart is limited by the vagus, which, as we have seen, readily steps in so soon as the call for an increased supply of blood has ceased. It may do so earlier, presumably because the increased blood pressure or the fatigue of the heart calls for vagus intervention.

In Section XII we consider the mode of interaction of the vagi and augmentores; we point out that when the vagi are paralysed by section or atropin the augmentores have no control over the cardiac rhythm, and that therefore they can only act by inhibiting the influence of the vagi on the rhythmic centre of the heart. When neither nerve is acting on the auricles they contract with a certain force, which is increased by the augmentores and diminished or inhibited by the vagi. The force of the ventricular contractions is increased by augmentor action: this increase can be inhibited by vagus excitation, which latter has otherwise no power to reduce the strength of ventricular contractions.

The force of the heart's contractions is influenced by other factors than the vagi, augmentores, and other nerves. The pressure of the blood in the coronary arteries is one of the most important of these factors. If this be lowered, the contractions of both auricles and ventricles diminish in strength, while a rise of pressure in the systemic arteries causes an increase in the force of the heart's con-

tractions, so that the force of the heart's contractions is to a certain extent regulated automatically by changes in the blood pressure in the aorta, which is one of the variable quantities affecting the work of the left ventricle.

Change of the volume of blood in the body affects greatly the contraction volume and output of the heart. Injections into the veins of a volume of defibrinated blood equal to one-tenth of the total blood in the body may double the output. It is important to note here that there is no increase in the strength of the ventricular contractions; increase in the work, therefore, of the ventricles due to increase in the output has no tendency to automatically increase the force of the ventricular contractions, as is the case with rise of pressure in the systemic arteries. We refer to the bearing of this in cases of plethora.

Increase of the watery constituents of the blood increases the contraction volume and output to the same extent (though only temporarily) as does transfusion of blood, but acts more unfavourably on the heart, seeing that the work done by the ventricles is increased, while the nutritive value of the blood supplied to the coronaries is diminished.

The increased output of the heart both in plethora and in hydræmia is due to rise of pressure in the systemic veins increasing the volume of blood which enters the right ventricle during diastole. We refer to the bearing of these facts upon the treatment of chlorosis and heart disease.

In Section XIV we consider the limits of the power of the heart to perform the work thrown upon it, and show that in strictly physiological conditions, and in spite of the beautiful mechanism by which the force of the ventricular contraction is regulated, the heart, like the voluntary muscles of the body, is liable to fatigue when the work thrown upon it greatly exceeds that required to maintain the circulation under ordinary circumstances. We take as example the increased work thrown upon the organ during active muscular exertion, and show that exertion and endurance of fatigue are limited mainly by the limited power of the heart to continue supplying the increased amount of blood which is required by the acting voluntary muscles. We show that those luxuries which are forbidden or limited in "training," and which are known to hinder prolonged exertion, such as water, alcohol, tobacco, caffein, all directly weaken the force of the heart's contractions, and, in the case of water, place the organ under a disadvantage; also that fatigue of the heart leads to dilatation of the organ.

On comparing the power of fatigued ventricles to carry on increased work, as compared with well-nourished unfatigued ventricles, it is found that not only is the strengthening effect of the augmentor

nerves upon the individual contractions less in the former case, but also that the fatigued and therefore dilated heart is *per se* unfavourably placed for meeting increase in the work thrown upon it. An explanation is given of the reason why in heart disease failure takes place during exertion.

The part played by the vagus in protecting the diseased heart from harmful over-work is referred to, and it is shown that irregularity of the heart in disease may be explained by the mode in which this nerve, when acting powerfully, releases the ventricles from the control of the rhythmic centre in the sinus. The chief forms of rhythmic and arrhythmic irregularity are considered, and it is shown that these correspond with the forms of irregularity which can be produced by vagus action. The irregular heart expends more energy, and its tissues therefore are more wasted, for a given amount of work than the heart which is beating regularly.

The effect upon the heart of imperfect aëration of the blood is, first of all, to produce powerful vagus action from the medullary centre; this is usually, though not always, accompanied in curarised animals by diminution of the output of the heart. But reasons are given for assuming that the output would be increased in uncurarised animals, owing to the high venous pressure which results from struggling. Besides the vagus action, it can be shown that asphyxia causes progressive weakening both of the auricles and of the ventricles, and attention is drawn to the fact that the considerable rise of pressure in the systemic arteries in asphyxia is accompanied by vagus effects upon the heart, and not by augmentor action, as is the case, so far as we know, in all other instances in which the vaso-constrictor centre is excited in the normal individual.

It is noted that the change in the heart and circulation which takes place during asphyxia points to the conclusion that, when the total amount of oxygen in the blood is lowered, it is for the benefit of the economy that those organs, such as the central nervous system, whose continuous blood supply is a vital necessity, should be richly furnished with blood by constriction of the vessels of the spleen, kidney, and digestive system, whose blood supply can be cut off temporarily without danger to life, and also that the heart should carry on the circulation in a manner involving as little as possible waste of its own substance. This, as we have seen, it is the function of the vagus nerve to bring about.

III. "The Rôle played by Sugar in the Animal Economy. Preliminary Note on the Behaviour of Sugar in Blood." By VAUGHAN HARLEY, M.D. Communicated by GEORGE HARLEY, M.D., F.R.S. Received January 4, 1892.

Presents, February 11, 1892.

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Map of England showing Lines of Equal Magnetic Declination for January 1, 1892. The Editor, 'Colliery Guardian.'

Reduced Photograph of Map showing the Points of Earthquake Frequency in New Zealand. Mr. George Hogben, Timaru.

February 18, 1892.

The LORD KELVIN, D.C.L., LL.D., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Nature of the Shoulder Girdle and Clavicular Arch in Sauropterygia." By H. G. SEELEY, F.R.S. Received January 18, 1892.

[Publication deferred.]

II. "On the Origin from the Spinal Cord of the Cervical and Upper Thoracic Sympathetic Fibres, with some Observations on White and Grey Rami Communicantes." By J. N. LANGLEY, M.A., F.R.S., Fellow and Lecturer of Trinity College, Cambridge. Received January 20, 1892.

(Abstract.)

The experiments of which an account is given in this paper were made upon anæsthetised cats, dogs, and rabbits. The lower cervical and upper thoracic nerves were tied, cut, and stimulated in the

vertebral canal, and the effects of the stimulation observed. The results were as follows:—

None of the lower cervical nerves produces any of the effects which can be produced by stimulating the upper thoracic or cervical sympathetic; *i.e.*, the lower cervical nerves send no efferent visceral fibres to the sympathetic.

The *pupil* receives dilator fibres from the 1st, 2nd, and 3rd thoracic nerves. The relative effect of these nerves upon the pupil varies somewhat in different animals of the same species, and varies considerably in animals of different orders. In the cat and dog, both the 1st and 2nd thoracic nerves cause great dilation of the pupil; in the cat, as a rule, the 1st produces greater dilation than the 2nd thoracic, but this is not always the case, and sometimes the 2nd is more powerful than the 1st thoracic nerve; the 3rd thoracic nerve has a comparatively slight action, and the extent of its action varies: in some cases the dilation produced by it is readily observed, in others it requires special attention. In the rabbit, the 2nd thoracic nerve is the chief dilator nerve for the pupil; the 3rd thoracic nerve produces a considerable dilation, but less promptly than the 2nd; the 1st thoracic has the least action of the three, and in some cases has a very slight effect.

The nerve-fibres causing *retraction of the nictitating membrane and opening of the eyelids* have in the dog and rabbit the same origin as the dilator fibres for the pupil. In the cat, their origin is somewhat more extended; a few fibres arise from the 4th thoracic nerve, and occasionally a very few from the 5th thoracic nerve.

The *vaso-motor fibres for the head** arise in the cat from the first five thoracic nerves, in the dog from the first four, and probably to a slight extent also from the 5th. The 1st thoracic nerve has a slight to moderate vaso-motor effect in the dog, a less and inconstant effect in the cat; the 2nd and 3rd thoracic nerves cause complete and rapid constriction of the small arteries on the same side of the head; the 4th thoracic also causes complete contraction, but more slowly than either the 3rd or the 2nd: in the dog its effect is less than in the cat; the 5th thoracic nerve has in the cat a distinct though less effect than the 4th; in the dog its action is doubtful.

In the rabbit, the vaso-motor nerves for the ear arise from the 2nd to the 8th thoracic nerves inclusive; the 5th nerve has usually the most rapid effect; passing upwards or downwards, the effect decreases; the 2nd and 8th nerves usually cause complete constriction in a part only of the auricular artery.

The *secretory fibres for the sub-maxillary gland* of the cat and dog have the same origin as the vaso-motor fibres for the head. The 2nd thoracic causes secretion more readily than any other nerve.

* Certain parts only of the head have been observed.

The *cardiac accelerator fibres* arise in the cat from the first four or five thoracic nerves; the maximum effect is obtained sometimes from the 2nd and sometimes from the 3rd thoracic nerve; the 1st and the 4th thoracic nerves have in some animals a considerable accelerator action, in others little or none; the 5th nerve appears occasionally to contain a few accelerator fibres, but further evidence is desirable.

Taking into account the pilo-motor fibres of the cat and dog, it is seen that the cervical sympathetic arises in these animals from the first seven, and in the rabbit from the first eight, thoracic nerves; the 1st thoracic is, however, less represented in the cervical sympathetic of the rabbit than it is in that of the cat and dog.

Comparing the rabbit with the cat and dog, as regards sympathetic fibres, which are present in all, it results that in the cat and dog the fibres of any one kind are higher in origin, and in some cases present in fewer spinal nerves, than they are in the rabbit. In accordance with this, the 2nd thoracic more frequently causes a movement of the fore-foot in the rabbit than in the other two animals. On the whole, the sympathetic fibres of any one kind appear to be slightly higher in the dog than in the cat.

The uppermost white ramus communicans arises from the 1st thoracic nerve; the lowest in the dog and cat arises usually, as described by Gaskell, from the 4th lumbar nerve; occasionally, however, the 5th lumbar nerve gives off a white ramus to the sympathetic. Both in the upper and lower regions of the spinal cord, there is satisfactory experimental evidence of efferent sympathetic fibres in those spinal nerves which have white rami, and in those only. This is in agreement with the views of Gaskell.

In the grey rami, medullated fibres of greater diameter than 4 μ—and, perhaps, some of the smaller ones—are probably afferent fibres, which pass to the spinal cord by the white rami.

A comparison of the histological characters and of the reflex effects yielded by various parts of the sympathetic, by the depressor, and by the nervus erigens, affords strong evidence that a considerable number of the medullated fibres of larger diameter than 4 μ, although afferent, are not fibres of general sensibility.

In the course of the paper the results of previous observers are given and discussed.

III. "On the Relative Densities of Hydrogen and Oxygen. II." By Lord Rayleigh, Sec. R.S. Received February 5, 1892.

In a preliminary notice upon this subject,* I explained the procedure by which I found as the ratio of densities 15·884. The

* 'Roy. Soc. Proc.,' vol. 43, p. 356, February, 1888.

hydrogen was prepared from zinc and sulphuric, or from zinc and hydrochloric, acid, and was liberated upon a platinum plate, the generator being in fact a Smee cell, enclosed in a vessel capable of sustaining a vacuum, and set in action by closing the electric circuit at an external contact. The hydrogen thus prepared was purified by corrosive sublimate and potash, and desiccated by passage through a long tube packed with phosphoric anhydride. The oxygen was from chlorate of potash, or from mixed chlorates of potash and soda.

In a subsequent paper on the Composition of Water,* I attacked the problem by a direct synthesis of water from weighed quantities of the two component gases. The ratio of atomic weights thus obtained was 15·89.

At the time when these researches were commenced, the latest work bearing upon the subject dated from 1845, and the number then accepted was 15·96. There was, however, nothing to show that the true ratio really deviated from the 16 : 1 of Prout's law, and the main object of my work was to ascertain whether or not such deviation existed. About the year 1888, however, a revival of interest in this question manifested itself, especially in the United States, and several results of importance have been published. Thus, Professor Cooke and Mr. T. W. Richards found a number which, when corrected for an error of weighing that had at first been overlooked, became 15·869.

The substantial agreement of this number with those obtained by myself seemed at first to settle the question, but almost immediately afterwards there appeared an account of a research by Mr. Keiser, who used a method presenting some excellent features, and whose result was as high as 15·949. The discrepancy has not been fully explained, but subsequent numbers agree more nearly with the lower value. Thus, Noyes obtains 15·896, and Dittmar and Henderson give 15·866.

I had intended further to elaborate and extend my observations on the synthesis of water from weighed quantities of oxygen and hydrogen, but the publication of Professor E. W. Morley's masterly researches upon the "Volumetric Composition of Water"† led me to the conclusion that the best contribution that I could now make to the subject would be by the further determination of the relative densities of the two gases. The combination of this with the number 2·0002,‡ obtained by Morley as the mean of astonishingly concordant

* 'Roy. Soc. Proc.,' vol. 45, p. 425, February, 1889.

† 'Amer. Journ. Sci.,' March, 1891.

‡ It should not be overlooked that this number is difficult to reconcile with views generally held as to the applicability of Avogadro's law to very rare gases. From what we know of the behaviour of oxygen and hydrogen gases under compression, it seems improbable that volumes which are as 2·0002 : 1 under atmo-

individual experiments, would give a better result for the atomic weights than any I could hope to obtain directly.

In all work of this sort, the errors to be contended with may be classed as either systematic or casual. The latter are eliminated by repetition, and are usually of no importance in the final mean. It is systematic errors that are most to be dreaded. But although directly of but little account, casual errors greatly embarrass a research by rendering difficult and tedious the detection of systematic errors. Thus, in the present case, almost the only source of error that can prejudice the final result is impurity in the gases, especially in the hydrogen. The better the hydrogen, the lighter it will prove; but the discrimination is blunted by the inevitable errors of weighing. After perhaps a week's work it may become clear that the hydrogen is a little at fault, as happened in one case from penetration of nitrogen between the sealed-in platinum electrodes and the glass of the generator.

Another difficulty, which affects the presentation of results, turns upon the one-sided character of the errors most to be feared. As has been said, impure hydrogen can only be too heavy, and another important source of error, depending upon imperfect establishment of equilibrium of pressure between the contents of the globe and the external atmosphere, also works one-sidedly in the same direction. The latter source of error is most to be feared immediately after a re-greasing of the tap of the globe. The superfluous grease finds its way into the perforation of the plug, and partially blocks the passage, so that the six minutes usually allowed for the escape of the initial excess of pressure in the globe may become inadequate. Partly from this cause and partly from incomplete washing out of nitrogen from the generator, the first filling of a set was so often found abnormally heavy that it became a rule in all cases to reject it. From these and other causes, such as accidental leakages not discovered at the time, it was difficult to secure a set of determinations in which the mean really represented the most probable value. At the same time, any arbitrary rejection of individual results must be avoided as far as possible.

In the present work two objects have been especially kept in view. The first is simplicity upon the chemical side, and the second the use of materials in such a form that the elimination of impurities goes forward in the normal working of the process. When, as in the former determinations, the hydrogen is made from zinc, any impurity which that material may contain and communicate to the gas cannot be eliminated from the generator; for each experiment

spheric conditions would remain as 2 : 1 upon indefinite expansion. According to the formula of Van der Waals, a greater change than this in the ratio of volumes is to be expected.

brings into play a fresh quantity of zinc, with its accompanying contamination. Moreover, the supply of acid that can be included in one charge of the generator is inadequate, and good results are only obtained as the charge is becoming exhausted. These difficulties are avoided when zinc is discarded. The only material consumed during the experiments is then the water, of which a large quantity can be included from the first. On the other hand, the hydrogen liberated is necessarily contaminated with oxygen, and this must be removed by copper contained in a red-hot tube. In the experiments to be described the generator was charged with potash,* and the gases were liberated at platinum electrodes. In the case of a hydrogen filling the oxygen blew off on one side from a mercury seal, and on the other the hydrogen was conveyed through hot tubes containing copper. The bulk of the aqueous vapour was deposited in a small flask containing strong solution of potash, and the gas then passed over solid potash to a long tube packed with phosphoric anhydride. Of this only a very short length showed signs of being affected at the close of all operations.

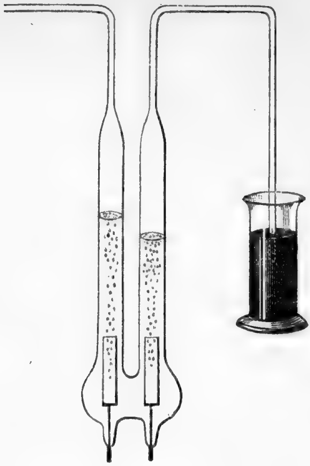
With respect to impurities, other than oxygen and oxides of hydrogen, which may contaminate the gas, we have the following alternative. Either the impurity is evolved much more rapidly than in proportion to the consumption of water in the generator, or it is not. If the rate of evolution of the impurity, reckoned as a fraction of the quantity originally present, is not much more rapid than the correspondingly reckoned consumption of water, the presence of the impurity will be of little importance. If on the other hand, as is probable, the rate of evolution is much more rapid than the consumption of water, the impurity is soon eliminated from the residue, and the gas subsequently generated becomes practically pure. A similar argument holds good if the source of the impurity be in the copper, or even in the phosphoric anhydride; and it applies with increased force when at the close of one set of operations the generator is replenished by the mere addition of water. It is, however, here assumed that the apparatus itself is perfectly tight.

Except for the reversal of the electric current, the action of the apparatus is almost the same whether oxygen or hydrogen is to be collected. In the latter case the copper in the hot tubes is in the reduced, and in the former case in the oxidised, state. For the sake of distinctness we will suppose that the globe is to be filled with hydrogen.

The generator itself (fig. 1) is of the U-form, with unusually long branches, and it is supplied from Grove cells with about 3 ampères of electric current. Since on one side the oxygen blows off into the

* At the suggestion of Professor Morley, the solution was freed from carbonate, or nearly so, by the use of baryta, of which it contained a slight excess.

Fig. 1.



air, the pressure in the generator is always nearly atmospheric. Some trouble has been caused by leakage between the platinum electrodes and the glass. In the later experiments to be here recorded these joints were drowned with mercury. On leaving the generator the hydrogen traverses a red-hot tube of hard glass charged with copper,* then a flask containing a strong solution of potash, and afterwards a second similar hot tube. The additional tube was introduced with the idea that the action of the hot copper in promoting the union of the hydrogen with its oxygen contamination might be more complete after removal of the greater part of the oxygen, whether in the combined or in the uncombined state. From this point onward the gas was nearly dry. In the earlier experiments the junctions of the hard furnace tubes with the soft glass of the remainder of the apparatus were effected by fusion. One of these joints remained in use, but the others were replaced by india-rubber connexions *drowned in mercury.* It is believed that no leakage occurred at these joints; but as an additional security a tap was provided between the generator and the furnace, and was kept closed whenever there was no forward current of hydrogen. In this way the liquid in the generator would be protected from any possible infiltration of nitrogen. Any that might find its way into the furnace tubes could easily be removed before the commencement of a filling.

Almost immediately upon leaving the furnace tubes the gas arrives

* The copper must be free from sulphur; otherwise the contamination with sulphuretted hydrogen is somewhat persistent.

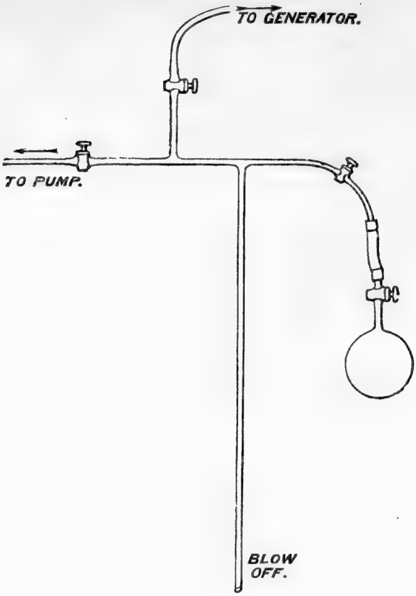
at a tap which for distinctness may be called the regulator. In the generator and in the furnace tubes the pressure must be nearly atmospheric, but in the globe there is (at the commencement) a vacuum. The transition from the one pressure to the other takes place at the regulator, which must be so adjusted that the flow through it is approximately equal to the production of gas. At first the manipulation of the regulator was a source of trouble and required almost constant attention, but a very simple addition gave the desired control. This was merely a long wooden arm, attached to the plug, which served both as a lever and as an indicator. Underneath the pointed extremity was a small table to which its motions could be referred. During the first two-thirds of a filling very little readjustment was needed, and the apparatus could be left for half an hour with but little fear of displacing too much the liquid in the generator. Towards the close, as the motive force fell off, the tap required to be opened more widely. Sometimes the recovery of level could be more conveniently effected by insertion of resistance into the electric circuit, or by interrupting it altogether for a few minutes. Into details of this kind it is hardly necessary to go further.

From the regulator the gas passed to the desiccating tubes. The first of these was charged with fragments of solid potash, and the second with a long length of phosphoric anhydride. Finally, a tube stuffed with glass wool intercepted any suspended matter that might have been carried forward.

The connexion of the globe with the generator, with the Töppler, and with the blow-off, is shown in the accompanying fig. 2. On the morning of a projected filling the vacuous globe would be connected with the free end of the stout-walled india-rubber tube, and secured by binding wire. The generator being cut off, a high vacuum would be made up to the tap of the globe. After a couple of hours' standing the leakage through the india-rubber and at the joints could be measured. The amount of the leakage found in the first two hours was usually negligible, considered as an addition to a globeful of hydrogen, and the leakage that would occur in the hours following would (in the absence of accidents) be still smaller. If the test were satisfactory, the filling would proceed as follows :—

The electric current through the generator being established and the furnace being heated, any oxygen that might have percolated into the drying tubes had first to be washed out. In order to do this more effectively, a moderate vacuum (of pressure equal to about 1 inch of mercury) was maintained in the tubes and up to the regulator by the action of the pump. In this way the current of gas is made very rapid, and the half hour allowed must have been more than sufficient for the purpose. The generator was then temporarily cut off, and a high vacuum produced in the globe connexion and in the blow-off

FIG. 2.



tube, which, being out of the main current of gas, might be supposed to harbour impurities. After this the pump would be cut off, the connexion with the generator re-established, and, finally, the tap of the globe cautiously opened.

The operation of filling usually occupied from two to three hours. When the gas began to blow off under an excess of pressure represented by about half an inch of mercury, the blow-off cistern was lowered so as to leave the extremity of the tube free. For two minutes the current of gas from the generator was allowed to flow through, after which the generator was cut off, and the globe left in simple communication with the atmosphere, until it was supposed that equilibrium of pressure had been sufficiently established. Doubts have at various times been felt as to the interval required for this purpose. If too little time is allowed, there will remain an excess of pressure in the globe, and the calculated weight of the filling will come out too high. On the other hand, an undue prolongation of the time might lead to a diffusion of air back into the globe. In a special experiment no abnormal weight was detected after half an hour's communication, so that the danger on this side appeared to be small. When the passages through the taps were free from grease, one or

two minutes sufficed for the establishment of equilibrium, but there was always a possibility of a partial obstruction. In the results to be presently given four minutes were allowed after the separation from the generator. It may be remarked that a part of any minute error that may arise from this source will be eliminated in the comparison with oxygen, which was collected under like conditions.

The reading of the barometers and thermometers at the moment when the tap of the globe was turned off took place as described in the former paper. The arrangements for the weighings were also the same.

In the evacuations the process was always continued until, as tested by the gauge of the Töppler after at least a quarter of an hour's standing, the residue could be neglected. Here, again, any minute error would be eliminated in the comparison of the two gases.

In the case of oxygen, the errors due to contamination (even with hydrogen) are very much diminished, and similar errors of weighing tell very much less upon the proportional agreement of the final numbers. A comparison of the actual results with the two kinds of gas does not, however, show so great an advantage on the side of the oxygen as might have been expected. The inference appears to be that the individual results are somewhat largely affected by temperature errors. Two thermometers were, indeed, used (on opposite sides) within the wooden box by which the globe is surrounded, and they could easily be read to within $\frac{1}{20}^\circ$ C. But in other respects, the circumstances were unfavourable in consequence of the presence in the same room of the furnace necessary to heat the copper. An error of $\pm 0{\cdot}1^\circ$ C. in the temperature leads to a discrepancy of 1 part in 1500 in the final numbers. Some further elaboration of the screening arrangements actually employed would have been an improvement, but inasmuch as the circumstances were precisely the same for the two gases, no systematic error can here arise. The thermometers were, of course, the same in the two cases.

The experiments are grouped in five sets, two for oxygen and three for hydrogen. In each set the work was usually continued until the tap of the globe required re-greasing, or until, owing to a breakage or to some other accident, operations had to be suspended.

Oxygen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.
	grams.	°	°
June 29	2·5182	70	20·85
July 2	2·5173	69	20·60
July 4	2·5172	67½	19·75
July 6	2·5193	70½	21·40
July 9	2·5174	64	17·60
July 10	2·5177	65½	19·05
Mean	2·51785	68	20°

The six fillings were all independent, except that of July 6, when the bulk of the oxygen remaining from the previous filling was not removed. It so happens that this case shows the greatest discrepancy, but there seems to be no sufficient reason for rejecting it.

Hydrogen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.
	gram.	°	°
July 31	0·15807	60½	15·90
August 4	0·15816	65	18·00
August 6	0·15811	66½	19·20
August 8	0·15803	65	18·15
August 11	0·15801	66	19·15
August 13	0·15809	68½	20·10
Mean	0·15808	65	18°

Hydrogen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.
	gram	°	°
September 22	0·15800	58	14·5
September 24	0·15820	61½	16·3
September 28	0·15792	62	17·6
September 30	0·15788	63½	18·1
October 2	0·15783	62	17·3
Mean	0·15797	61	17°

Hydrogen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.
	gram.		
October 26	0·15807	55°	13°·30
October 28	0·15801	56	14·00
October 31	0·15817	50	10·95
November 3	0·15790	53½	12·10
November 5	0·15810	55	12·00
November 7	0·15798	50	10·70
November 10	0·15802	48	9·30
November 13	0·15807	55½	12·70
Mean	0·15804	53	12°

Oxygen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.
	grams.		
November 31	2·5183	53°	12°·15
December 3	2·5168	56	13·55
December 5	2·5172	56½	14·15
December 7	2·5181	58½	14·70
December 8	2·5156	51	11·15
Mean	2·5172	55	13°

In almost every case the weight of the globe *full* is compared with the mean of the immediately preceding and following weights *empty*. The numbers recorded in the second column are derived from the readings of the balance by the introduction of corrections—

(1.) For the errors of the weights themselves, found by a systematic comparison, only relative values uncorrected for buoyancy being required.

(2.) For the deviation of the mean* barometric reading at the time of filling from 30 inches (as read upon the vernier).

(3.) For the deviation of the temperature of the barometers (Column 3) from 60° F.

(4.) For the deviation of the temperature of the gas (as read upon the thermometers) from 12° C.

As an example, I will take in detail the calculation for the hydrogen filling of October 26. After the evacuation of October 24, the working globe (14) with its compensating volume piece and

* There were two barometers.

0·4778 gram stood on the left of the balance with globe (11) on the right. The position of equilibrium of the pointer, as determined after four different releasements, each observed in the usual manner, was 19·02 scale divisions. In like manner, after the evacuation of October 27, with the same weights in use, the equilibrium position of the pointer was 18·46. After the filling of October 26, the weights associated with (14) were 0·3220 gram, instead of 0·4778; and the pointer reading was 20·08. So far as the weights are concerned, the value of the hydrogen would be 0·4778 − 0·3220, or 0·1558 gram; but to this we must add a correction corresponding to 1·34 scale divisions, being the difference between 20·08 and $\frac{1}{2}(19{\cdot}02 + 18{\cdot}46)$. At the time in question, the value of a scale division was 0·00020 gram, so that we obtain—

$$0{\cdot}1558 + 0{\cdot}00027 = 0{\cdot}15607.$$

The particular weights in use on this occasion were such that no correction is necessary in order to allow for their errors.

The mean barometer reading at the time of filling was 29·742, so that the factor required on this account is 30 : 29·742. The correction for temperature of gas is from 13·3 to 12°.

Log 0·15607........	= $\bar{1}{\cdot}19332$
For barometer......	0·00375
For temperature	0·00198
Log 0·15814........	= $\bar{1}{\cdot}19905$

To this a correction for the temperature of the *barometer* has still to be applied. For 1° F. the correcting factor is (1 − 0·000089), or for 5° F. (1 − 0·000445). From 0·15814 we are thus to subtract 0·00007, giving the tabular number 0·15807.

A further minute correction to the mean of each set may be made for the temperature of the glass. A warm globe is larger than a cold one, and consequently holds more gas. If we suppose that the volume expansion of the glass per degree C. is 0·000025, we find, corrected to 12° C.—

Hydrogen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.	Corrected to 12°.
	gram.	°	°	gram.
July..............	0·15808	65	18	0·158056
September	0·15797	61	17	0·157950
October	0·15804	53	12	0·158040
Mean		60	16	0·158015

Oxygen.

1891.	Weight.	Bar. temp., F.	Globe temp., C.	Corrected to 12°.
	grams.	°	°	grams.
June	2·51785	68	20	2·51735
November	2·51720	55	13	2·51713
Mean		$61\frac{1}{2}$	$16\frac{1}{2}$	2·57124

The means here exhibited give the weights of the two gases as they would be found with the globe at 12° C., and the barometers at 60° F. and at 30 inches. The close agreement of the mean temperatures for the two gases shows how little room there is for systematic error dependent upon imperfections in the barometers and thermometers. But the results still require modification before they can be compared with the view of deducing the relative densities of the gases.

In the first place, there is a systematic, though minute, difference in the pressures hitherto considered as corresponding. The terminal of the blow-off tube is 33 inches below the centre of the globe at the time of filling. In the one case this is occupied by hydrogen, and in the other by oxygen. If we treat the latter as the standard, we must regard the hydrogen fillings as taking place under an excess of pressure equal to $\frac{15}{16}$ of the weight of a column of oxygen 33 inches high; and this must be compared with 30 inches of mercury. Hence, if we take the sp. gr. of oxygen under atmospheric conditions at 0·0014, and that of mercury at 13·6, the excess of pressure under which the hydrogen was collected is as a fraction of the whole pressure

$$\frac{33}{30} \cdot \frac{15}{16} \cdot \frac{0{\cdot}0014}{13{\cdot}6} = 0{\cdot}000106;$$

and $0{\cdot}000106 \times 0{\cdot}158 = 0{\cdot}000017$. This, then, is what we must subtract from the weight of the hydrogen on account of the difference of pressures due to the gas in the blow-off tube. Thus

$$H = 0{\cdot}157998, \qquad O = 2{\cdot}51724.$$

[These numbers are not quite comparable with those given in the former communication, inasmuch as the standard temperature then used *for the barometers* was 55° F. Reduced so as to correspond to 60°, the former numbers become

$$H = 0{\cdot}15797, \qquad O = 2{\cdot}5174.$$

The agreement is satisfactory, especially when it is remembered that both gases were prepared by different methods in the two sets of experiments.—Feb. 17.]

But there is still another and a more important correction to be introduced. In my former paper it was shown that when the weighings are conducted in air the true weight of the gas contained in the globe is not given by merely subtracting the weight of the globe when empty from the weight when full. When the globe is empty, its external volume is less than when full, and thus, in order to obtain the true weight, the apparent weight of the gas must be increased by the weight of air whose volume is equal to the change of volume of the globe.

In order to determine the amount of this change of volume, the globe is filled to the neck with recently boiled distilled water, and the effect is observed upon the level in the stem due to a suction of, say, 20 inches of mercury. It is not advisable to carry the exhaustion much further for fear of approaching too nearly the point at which bubbles of vapour may be formed internally. In the earlier experiments, described in the preliminary note, the upper surface of the liquid was in the stem of the globe itself (below the tap), and the only difficulty lay in the accurate estimation of a change of volume occurring in a wide and somewhat irregular tube. The method employed was to produce, by introduction of a weighed quantity of mercury, a rise of level equal to that caused by the suction.

The advantage of this procedure lay in the avoidance of joints and of the tap itself, but, for the reasons given, the readings were not quite so accurate as might be desired. I wished, therefore, to supplement, if possible, the former determination by one in which the change of volume occurred in a tube narrower and of better shape. With this object in view, the stem of the globe was prolonged by a graduated tubular pipette attached with the aid of india-rubber. The tubes themselves were treated with gutta-percha cement, and brought almost into contact. It had hardly been expected that the joint would prove unyielding under the applied suction, but it was considered that the amount of the yielding could be estimated and allowed for by operations conducted *with tap closed.* The event, however, proved that the yielding at the joint was scarcely, if at all, perceptible.

The pipette, of bore such that 16 cm. corresponded to 1 c.c., was graduated to 0·01, and was read by estimation to 0·001 c.c. In order the better to eliminate the changes due to temperature, readings under atmospheric pressure, and under a suction of 20 inches of mercury, were alternated. On January 28, 1892, a first set gave $0{\cdot}648 - 0{\cdot}300 = 0{\cdot}348$, a second gave $0{\cdot}6645 - 0{\cdot}316 = 0{\cdot}3485$ and a third gave

0·675 − 0·326 = 0·349. Similar operations with tap closed* gave no visible movement.

The result of the day's experiments was thus 0·3485 for 20 inches, or 0·523 for 30 inches, suction. Similar experiments on January 28, at a different part of the graduation, gave 0·526. On this day the yielding with tap closed was just visible, and was estimated at 0·001. As a mean result, we may adopt 0·524 c.c. The graduation of the pipette was subsequently verified by weighing a thread of mercury that occupied a measured length.

A part of the above-measured volume is due to the expansion of the water when the pressure is relieved. We may take this at 0·000047 of the volume per atmosphere. The volume itself may be derived with sufficient accuracy for the present purpose from the weight of its oxygen contents. It is 2·517/0·00137, or 1837 c.c. The expansion of the water per atmosphere is thus 0·000047 × 1837, or 0·087 c.c. This is to be subtracted from 0·524, and leaves 0·437 c.c. This number applies strictly to the volume enclosed within the glass, but the change in the external volume of the globe will be almost the same.†

The correction now under consideration is thus the weight of 0·437 c.c. of air at the average temperature of the balance room. The density of this air may be estimated at 0·00122; so that the weight of 0·437 c.c. is 0·000533 gram. This is the quantity which must be added to the apparent weights of the gases. The former estimate was 0·00056 gram. The finally corrected weights are thus

$$H = 0·158531, \qquad O = 2·51777;$$

and for the ratio of densities we have

15·882.

This corresponds to a mean atmospheric condition of pressure and temperature.

If we combine the above ratio of densities with Professor Morley's ratio of volumes, viz., 2·0002 : 1, we get, as the ratio of atomic weights, 15·880.

If we refer to the table, we see that the agreement of the first and

* For greater security the tap was turned while the interior was under suction.

† For a spherical shell of glass of uniform thickness and with elastic constants following Poisson's law, the ratio of the difference of the internal and external expansion to either of them is $4t/3a$, where t is the thickness of the shell, and a the mean radius. In the present application the value of a/t, deduced from the measured circumference and from the weight of glass, is about 110.

[Perhaps an arrangement in which the *external* volume is directly measured would have been preferable. No allowance for expansion of water would then be needed.—Feb. 17.]

third series of hydrogen weighings is very good, but that the mean from the second series is decidedly lighter. This may have been in part fortuitous, but it is scarcely probable that it was so altogether. Under the circumstances we can hardly reckon the accuracy of the final results as closer than $\frac{1}{3000}$.

A word should perhaps be said upon a possible source of systematic error, viz., mercury vapour. There is no doubt that hydrogen passed over mercury takes up enough to cause a slow and superficial, but quite distinct, discoloration of sulphur over which it subsequently flows. In the experiments here recorded, the gas did not, indeed, flow over mercury in mass, but, inasmuch as mercury was used to secure the tightness of some of the joints, it is difficult to feel sure of its absence. Again, in evacuations conducted with a mercury pump can the vacuum be regarded as free from mercury vapour, which, it must be remembered, would not show itself upon the gauge of the Töppler? If both the hydrogen and the "vacuum" were saturated with mercury vapour, the result of the weighings would, according to Dalton's law, be free from its influence. The same may be said of any volatile impurity arising from the grease* upon the stopcocks. As the matter stands, the results must, I think, be regarded as affected with a possible error amounting to a fraction of the weight of mercury vapour at the temperatures employed. But this is probably a very small quantity.

According to Hertz,† the vapour-pressure of mercury at 15° C. would be about 0·001 mm. If this be correct, the weight of mercury vapour in an atmosphere of hydrogen would be as a fraction of the latter

$$\frac{0{\cdot}001}{760} \times 200 = \frac{1}{3800}.$$

It appears that in an investigation of hydrogen aiming at an accuracy of 1/10,000 the question of mercury vapour requires very careful consideration.

The accompanying table of results found by various experimenters may be useful for comparison :—

* Composed of vaseline and beeswax.

† 'Wied. Ann.,' vol. 17, p. 199.

Name.	Date.	Atomic weights.	Densities.
Dumas	1842	15·96	—
Regnault	1845	—	15 96
Rayleigh	1888	—	15·884
Cooke and Richards........	1888	15 869	—
Keiser....................	1888	15·949	—
Rayleigh	1889	15·89	—
Noyes	1890	15·896	—
Dittmar	1890	15·866	—
Morley	1891	15·879	—
Leduc.....................	1891	—	15·905
Rayleigh	1892	—	15·882

In conclusion, I must express my obligations to Mr. Gordon, who has assisted me throughout. The work has been unusually tedious, partly from its inherent nature, requiring as it does a certainty of 0·1 milligram in the weighings, and still more from the constant liability to accidents, which may render nugatory a large amount of preparatory work.

Presents, February 18, 1892.

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February 25, 1892.

Mr. JOHN EVANS, D.C.L., LL.D., Treasurer, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Note on Nova Aurigæ." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S., and Mrs. HUGGINS. Received February 24, 1892.

We have delayed up to the present time presenting any account of our observations of Nova Aurigæ, in the constant hope that fine weather would enable us to make our observations more complete. We think now, however, that it may be of interest without further delay to send a short preliminary notice of this remarkable, and, in some respects, unprecedented, celestial phenomenon. For up to this time we have no record of a star in the spectrum of which the bright and dark lines of the same substances have been regarded as indicating respectively motions of approach and of recession of so great magnitude. It was partly for this reason that we were anxious for the opportunity of observing if any change in the amount of relative motion would show itself.

We received a telegram from Dr. Copeland in the early morning of the 2nd instant, and began our observations of the star on the night of the 2nd instant.

Perhaps the most noticeable feature to the eye in the star's spectrum was the great brilliancy of the hydrogen lines at C, F, and G; but the point of greatest interest was obviously that two of these lines, F and G—and we have since observed the same with C—were accompanied each by a strong absorption line on the side towards the blue. Comparison with the lines of terrestrial hydrogen, while confirming the obvious presumption that the star-lines were really those of hydrogen, showed at once a large motion of recession of the bright lines and a motion of approach of a similar order of magnitude of the hydrogen which produced the absorption.

A photograph which we have since taken gives the star's spectrum as far in the ultra-violet as about λ 3200. On this plate we see not only the other hydrogen lines at *h* and H, but also the series beyond,

which is characteristic of the white stars, bright, with dark absorption lines on the blue side.

Besides the hydrogen series there appear to be other lines doubled in a similar manner, including the sodium lines at D. The line K, which is at least as strongly impressed upon the plate as H, is not followed by so strong an absorption.

In the green part of the spectrum three very brilliant lines are seen on the red side of F. One of these falls not far from the position of the chief nebular line; but even when the shift of the spectrum is taken into account, we can scarcely regard this line as the true nebular line. In this connexion it was a point of some importance to find that the strong and very characteristic line of the Orion nebula, which falls about λ 3725, is absent in our photograph of the Nova, also the strong line between α and β at about λ 3868.

[The third line from F is rather broad and resolvable into lines. It falls partly upon the more refrangible pair of the magnesium triplet at *b*, but its character and position do not permit us to ascribe it either to magnesium or carbon.—Feb. 25.]

We wish to mention an early photograph of this star taken on the 3rd instant by Father Sidgreaves, at Stonyhurst, which we had the privilege of examining. This successful photograph extends from about *h* to near D, and shows the remarkable doubling of many of the bright lines by dark ones, a feature which was at once noticed by Father Sidgreaves and ourselves.

In our photograph the spectrum of the star, which extends on the plate as far into the ultra-violet as our photographs of Sirius, is crowded throughout its entire length with dark and bright lines. In the visible region the number of bright lines and groups, including the double line of sodium, a fine line about the position of D_3, and lines on both sides of C, is also very great.

We prefer in this preliminary note not to enter into any more detailed discussion of the star's spectrum, nor to refer to the probable phenomena which may now be in progress in this celestial body. We reserve these considerations for the present.

II. "Note on the New Star in Auriga." By J. Norman Lockyer, F.R.S. Received February 25, 1892.

Since my note of February 11, observations of the new star have only been possible at Kensington on seven evenings, namely, February 11, 12, 13, 16, 22, 23, and 24. The 13th and 22nd were the only two very fine nights.

The star now appears to be fading. In the photograph of the region taken on February 3 the Nova appeared to be brighter than

χ Aurigæ (mag. 5·0), but in that taken on February 23 it is not brighter than the companion to this star, which is fainter than 6th magnitude. No marked diminution in brightness was noticed before February 22.

The colour has not appreciably changed since the star was first observed.

Photographs of the spectrum were attempted on all the dates named. Those of February 11, 12, 16, and 23, however, were insufficiently exposed, but they show that the dark lines were still more refrangible than the accompanying bright ones, and that the same lines were present as in the previous photographs. A plate was exposed for 2 hours 35 minutes on February 24, but no impression was obtained. The photograph taken on February 13 is identical with those referred to in the notes which I have already communicated to the Society. In the three photographs of February 22 there appears to be a slight diminution in the intensity of the H and K lines, but otherwise there is no decided change.

There is no evidence of revolution during the twenty days of observation. In all the photographs the dark lines are more refrangible than the bright ones, and the relative velocity deduced from those of February 3, 7, 13, and 22 appears to be about 600 miles per second. As this only represents the velocity in the line of sight, we are still ignorant of the real velocities of the two bodies. The constant relative velocity indicated by the displacement of the bright and dark lines may be regarded as confirming the supposition that two meteor-swarms or comets have collided, the velocities being so great, and the masses so small, that neither was captured by the other.

The relative velocity of 600 miles per second seems at first sight to be abnormally great, but, if we regard each of the component swarms as moving at the rate of 300 miles per second, the velocities are quite comparable with those of other bodies in space. The star 1830, Groombridge, for example, moves at the rate of 200 miles per second across the line of sight, and its real velocity may be much greater.

Eye observations have been made on every possible occasion. The chief variations from those previously reported are the general fading of the continuous spectrum and the consequent unmasking of the lines between *b* and D. Micrometric measures of four new lines in this region were made by Mr. Fowler on February 23 and 24. These, with the other lines observed at Kensington in the region F to C, are shown in the table which follows. The corresponding lines observed in the spectra of new stars which have previously appeared, and those in the spectra of some of the bright line stars, are added for comparison.

Nova Aurigæ. Feb. 23.	Nova Aurigæ. Feb. 24.	Nova Cygni. Cornu.	Nova Cygni. Vogel.	Nova Cygni. Copeland.	Nova Andromedæ.	Nova Coronæ.	γ Argus.	Arg. Oeltz. 17681.	Lalande, 13412.	Cygnus. 1st.	Cygnus. 2nd.	Cygnus. 3rd.	γ Cassiopeiæ (Sherman).	Suggested origins.
656 (C)	656	661	656	656	—	656	—	—	—	—	—	—	—	H (656·2).
635	630	635	630	630	—	—	—	—	—	—	636	636	635	
589 (D)	—	588	589	589	—	—	590	—	—	—	—	—	—	Na (589·1)
579	579	—	580	577·5	—	—	580·9	581	581	583	581	581	—	Fe (579·0)
—	570	—	—	—	—	—	—	—	—	571	570	569	—	
566	—	563	564	—	—	—	566	—	—	—	—	564	—	C (563·5)
558	558·3	—	—	—	558	—	—	—	—	558	—	558	556	Mn (557·6)
531	531·5	531	531	—	—	—	—	—	—	—	—	—	531	
518	517·7	518	—	—	517	—	—	—	—	—	—	517	517	C (516·5) or Mg (517·5)
500·6	500·6	500	499	502	—	501	—	—	—	—	—	—	499	Mg (500·6)
490	490·3	—	490	—	—	—	—	—	—	—	—	—	—	
486 (F)	486·2	483	486	486	486	486	—	—	—	—	—	—	486	H (486·2)

It will be seen that all the lines of Nova Aurigæ have previously been recorded in other Novæ, or in the bright-line stars.

The complete spectrum, including the photographic region, was shown in the diagram exhibited on the screen. This, and the light curve of the spectrum from F to C, was drawn by Mr. Fowler and Mr. W. J. Lockyer, on February 22, and confirmed by Mr. Fowler on February 23. The 3-foot reflector and McClean spectroscope were employed in each case.

The changes which are taking place in the Nova are exactly what would be expected according to my hypothesis, that new stars are produced by the collision of meteor-swarms. The rapid fading of the star demonstrates that small masses and not large ones are engaged, and this is further confirmed by the observed diminution in the brightness of the continuous spectrum relatively to the bright lines. If two condensed bodies were in collision, it is evident that the lines would fade first.

III. "On the Organisation of the Fossil Plants of the Coal-Measures. Part XIX." By W. C. WILLIAMSON, LL.D., F.R.S., Professor of Botany in the Owens College, Manchester. Received January 18, 1892.

(Abstract.)

The author recalls attention to the discovery by the late Rev. W. Vernon Harcourt of a fragment of a Lepidodendroid branch, in which the internal structures were well preserved. The specimen was described and figured, first by Witham, who gave to it the well-known name of *Lepidodendron Harcourtii.* It was next described by Lindley and Hutton, in their 'Fossil Flora,' and still later, and more scientifically, by Brongniart, in his 'Végétaux Fossiles.' In its interior Brongniart found a single vascular cylinder encasing a medulla. At a later period he obtained fragments of two other plants, in each of which he found the above cylinder, but invested by a second one which was obviously an exogenous product of a cambium zone. From these three specimens he unfortunately concluded that the first belonged to a Cryptogamic Lycopod, whilst the second and third were Gymnospermous Phanerogams. These latter examples he further identified with his genus *Sigillaria.*

This classification was universally accepted by the palæobotanical world until 1871, when, in his Memoir, Part II, the author announced his conviction that Lepidodendra and Sigillariæ were alike Cryptogams, and that the exogenous zone supposed to be characteristic of the Phanerogams was not confined, in ancient times, to that great division of the vegetable kingdom.

Apart from this general question, now conclusively settled, further knowledge of *L. Harcourtii* has long been sought for in vain. Harcourt's original fragment was unfortunately an imperfect one. Its outer cortex and foliage were wholly wanting, as well as specimens illustrating its various stages of growth. Recently, however, a very fine series of such specimens has come into the hands of the author, and a large amount of new information has been obtained from them. Some of the new examples are very young branches, perfectly invested by their bark and leaves. The detailed structures of all the organs of these specimens are now described in minute detail. A more exact technical nomenclature than has hitherto been employed is applied to their various structures. Besides these young forms, other specimens resembling that studied by Brongniart, both as regards condition and apparent age, have been obtained, and also one magnificent older and arborescent example, from Airdrie, in Scotland, which, including all its leaves, has been between four and five inches in diameter.

But even this latter specimen presents no appearance of the secondary or exogenous vascular zone so common amongst other much younger Lepidodendra. Hence the author concludes that *L. Harcourtii* has in this respect been like *L. Wunschianum*, the well-known Arran species, in which a magnificent exogenous zone exists, but which was only developed when the plants attained to an advanced arborescent condition.

Some of these youngest specimens show evidence that they had been fructigerous twigs. But, before describing these, the author examines anew the entire subject of the branches to which the names of *Halonia* and *Ulodendron* have been applied. Both of these have now been proved to have been fruit-bearing branches, but their true relations to each other and to their parent plants are still in a state of serious confusion. The existing definitions of these two types are shown to be altogether unsatisfactory; some specimens which according to one generally accepted definition are Haloniæ according to another are Ulodendra. In fact the two sets of definitions overlap in such a manner as renders them no longer applicable.

Two classes of facts have to be considered here; first, the positions and arrangements of the reproductive fructifications on the supporting branches, and, secondly, the nature of the scars left on the exterior of the bark after these deciduous fructifications fell to the ground. The positions of these scars in *Ulodendron* are usually defined as biserial, being arranged in two longitudinal rows,* one on each side of the sustaining branch, whilst in *Halonia* these

* In his last publication, M. Renault recognises that there are sometimes four such rows.

rows are defined as being more numerous, and the scars quincuncially arranged. In *Ulodendron* each such scar is further surrounded by a large circular, or oval, and very characteristic disk. The author shows that the essential and homologous structure in all these fruit-bearing branches is a small circular area, forming the summit of a larger or smaller conical arrested branch which was covered with leaves. This small apical area represents the part at which the deciduous fructification was organically united with its sustaining branch. Each such branch was supplied with a distinctive form of vascular bundle, which differed alike from those larger ones seen in ordinary vegetative branches, and from the smaller ones passing outwards to the leaves; this bundle is always abruptly broken off at the extreme apex of the fructigerous tubercle in a way demonstrating that it was formerly prolonged into some deciduous appendage which is rarely preserved *in situ*. But *Ulodendron* has, in addition, surrounding each of these fruit-bearing points, a flattened surface, the size of which was mainly dependent upon the age to which the tree had attained when it perished. This orbicular surface was primarily covered with ordinary leaves, normally arranged, but the full development of which was arrested by the pressure of some external agent. The author concludes that the central fructigerous point was homologous in all these cases, and that the variations seen in them arose largely from the degree of prominence attained by the arrested lateral branch. When that prominence was sufficient, the cone-like fruit was pedunculate, and no disturbance of the surrounding leaves was produced; but when that elevation was small, or almost non-existent, the cone was practically sessile, and, as it grew, its expanding base crushed down the leaves which it covered and thus produced the large flattened disk characteristic of *Ulodendron*. These two names, *Halonia* and *Ulodendron*, have no longer any generic value, but the terms Halonial and Ulodendroid may be conveniently retained as adjectives applicable to appropriate specific forms.

The author applies these conclusions to his younger specimens of *L. Harcourtii*, and shows that many of them were fructigerous in the Halonial form.

The organisation of the leaves of some of these Lepidodendroid plants has been re-examined. As is well known, on the leaf-scars alike of *Lepidodendron* and of *Sigillaria*, each scar left on the bark after the fall of the deciduous leaf had three minute points impressed upon its surface. Brongniart regarded each of the three as representing the entrance of a leaf-trace into the leaf, and very recently some other observers have arrived at the same conclusion. The author, long ago, showed that the central spot alone represented the vascular leaf-trace, the two lateral ones being merely cellular structures, but the details of which were very imperfectly known. These new speci-

mens demonstrate the exact features of these structures. Professor Bertrand, of Lille, and M. Hovelacque, of Paris, have simultaneously investigated the two lateral points on the leaf-scar, to which the former author has given the name of *parichnos*, which name Professor Williamson adopts. But these two palæontologists have further called attention to a fourth structure in these leaves, hitherto, in some degree, overlooked; and which they designated the *ligule.* The author finds this organ well developed both in *L. Harcourtii* and in *Lepidophloios*, but rejects the name ligule, on the ground that he cannot identify the fossil structure with the organ bearing the same name in living Isoetes and Selaginellæ. He, therefore, adopts for the former the term Adenoid organ, believing it to have had glandular functions. Details are also given of the organisation of several forms of Lepidostrobi, some of which are identified with their parent plants.

The general conclusion arrived at by the author in reference to the *L. Harcourtii*, which has been so often made the subject of debate during the last twenty years, is that it occupies no exceptional position amongst the other Lepidodendra, but that whilst palæontologists in various parts of the world quote the species as one with the organisation of which they were familiar, they were all alike mistaken in their determinations. Until now no specimen of the same plant has been in the possession of any observer less imperfect than that described by Brongniart; hence, when in the past authors have, as was my own case, referred various examples of cortex, leaves, and fruits to *Lepidodendron Harcourtii*, we have no evidence whatever that such references are true ones.

If such references are still declared to be authoritative, I must ask where the specimens are to be seen that carry our knowledge beyond what we derived from Harcourt's imperfect branch.

IV. "On Biologic Regions and Tabulation Areas." By C. B. Clarke, F.R.S. Received February 8, 1892.

(Abstract.)

Biologic regions have been used for two purposes, viz.: (1) to exhibit the most natural primary divisions of the globe, so far as the distribution of existing Mammalia (or of plants or living things) is concerned; (2) as areas of reference on which the complete distribution of a large genus or order of plants or animals may be tabulated.

It is clearly of the highest importance that one set of areas of reference should be employed by all naturalists, as foreseen by Mr. Wallace when he devised his primary zoologic regions and sub-regions. If one naturalist tabulates one order of Butterflies on one geographic

framework, and another naturalist tabulates another order of Butterflies on a different geographic framework, the results of the two naturalists can only be combined by doing the tabulation all over again, instead of by a simple addition.

Naturalists have not agreed on one system of primary reference areas; and, consequently, it is not possible to combine the results attained by different writers.

The first reason why naturalists have not accepted Wallace's recommendation is that his regions do not appear the most natural to many naturalists; Professor Huxley, Dr. Günther, and numerous botanists have proposed widely different regions as more natural. A second reason why these regions have not been used for tabulations is that their boundaries are (in many important cases) not accurately defined.

I have been for eighteen months past making trial of various geographic frameworks on which to tabulate the distribution of 2000 species of plants; and I have constructed a considerable number of maps, and have executed trial tabulations of a few genera on them. I have arrived at *one* conclusion which I deem of sufficient importance to bring before this Society, viz., that the two objects hitherto confounded in the designing of biologic regions must be kept entirely separate. Biologic regions representing the natural distribution of Mammalia or of life are *not* convenient to use as tabulation areas. I may venture to say, indeed, that the more perfectly natural the biologic regions are, and the more complex and detailed their boundary lines, the more impossible they are to use as reference areas or as tabulation areas on any considerable scale.

The idea of biologic regions presupposes a geographic framework of some kind on which the area of each genus of animals or plants was previously plotted. It appears to me that all naturalists, zoologists, botanists, and palæontologists, might easily agree to use one system of tabulation areas. Out of the results attained on this system, they might construct various biologic regions, each to please himself.

I have constructed, as a reference map for my own tabulations, the Map B. I would urge naturalists to use this, or that a committee be appointed to design a better, which should be put out by authority.

This Map B I have gradually arrived at by fixing down accurately the boundaries, and otherwise modifying the Map A, which is Wallace's map of zoologic regions. My object has been to make the smallest alterations in Wallace's map consistent with easy tabulation.

The greater part of the paper here abstracted is occupied with a detailed discussion of various boundary lines in the Map B, in order to bring out clearly the principles which should guide us in forming

our tabulation areas. One main object is that our primary framework of areas and sub-areas should separate our species and genera (so far as possible) into those areas and sub-areas; if a boundary line is drawn between two sub-areas A and B, so that nearly all the species found on one side of it are also found on the other, then we might as well, in this tabulation, have thrown the two sub-areas A and B into one, and saved ourselves labour. This brings us round practically pretty nearly to Wallace's view again; *i.e.*, geographic framework for reference and tabulation must be as near as possible to a system of natural biologic regions, subject to the condition that the boundary lines are rapidly and accurately fixed, and are easily remembered. It is impracticable to effect large tabulations of tens of thousands of specimens if it is necessary continually to refer to some special large-scale map.

The present paper is not intended to include marine regions or areas.

V. "The Electric Organ of the Skate: Observations on the Structure, Relations, Progressive Development, and Growth of the Electric Organ of the Skate." By J. C. EWART, M.D., Regius Professor of Natural History, University of Edinburgh. Communicated by Prof. J. BURDON SANDERSON, F.R.S. Received February 10, 1892.

(Abstract.)

After referring to the observations of Stark, the discoverer of the skate's electric organ, and to the work of Robin, Leydig, Babuchin, and others, the author describes the arrangement of the muscles in the tail of Selachians with a view to determining which muscles in the skate are transformed into the electric organs.

By comparing the caudal muscles of *Scyllium*, *Lamna*, *Myliobatis*, and *Raia*, it is made out that, while the middle row of muscular cones remains unaltered in the sharks and rays, it is transformed into a more or less perfect electric organ in the skates, the various members of the genus *Raia*. It is pointed out that, while the middle row of muscular cones is transformed in *Raia* into electric cones, the two adjacent rows of cones as in the rays and certain sharks diminish in size, and in some cases disappear about the middle of the tail.

In considering the structure of the organ, it is stated that, when the various modifications are taken into consideration, it may be described as consisting of a series of electric cones made up of more or less completely metamorphosed muscular fibres. Twenty-eight distinct cones were counted in the organ of *R. batis*. The first, which in a half grown fish measured 5 cm. in length, was all but

completely invested by the last unaltered muscular cone. From the first to the tenth the cones slightly increased in length; but from the eleventh they diminished in length, the twenty-sixth measuring only 0·75 cm. Beyond the twenty-eighth there were from six to eight incomplete cones.

In transverse sections the anterior third of the organ was seen to present an oval or rounded form, while the middle and posterior thirds were less regular, owing to the organ coming into contact with the vertebral column, and being grooved by the dorsal and ventral muscles.

The cones are described as consisting of numerous loculi or chambers, each having an electric disc suspended by nerve fibres from its anterior wall, and occupied in front and behind the disc with gelatinous tissue.

It is estimated that each organ in *R. batis* is made up of about 10,000 electric elements, *i.e.*, about 20,000 in the two organs. *Torpedo marmorata* has about 500,000, and *T. gigantea* about 1,000,000, elements in the two batteries, all considerably larger than those of the skate.

The layers of the electric discs, the electric, striated, and alveolar, are described in detail; and the various views as to the termination of the nerve fibrils in the disc are referred to.

In the chapter on the progressive growth of the organ a table is given to show that in *R. batis* the organ, after a time, grows at a greater rate than the tail in which it is lodged, *e.g.*, in fish 60 cm. in length the tail measures about 28 cm., and the electric organ 22·5 cm.; well-formed discs having an area of 0·8 to 1 sq. mm. In fish 225 cm. in length the tail measures 85 cm., the organ 70 cm., and the discs have an area of about 2·08 sq. mm. In fish from 25·5 to 30·5 cm. in length the organ is from 12·78 to 14·0 cm., and weighs 0·5 to 0·6 gram; in fish from 83·5 to 91·25 cm. the organ is from 30·50 to 34·25 cm., and weighs from 6·0 to 8·0 grams; in fish 157 cm., the organ measures 48·25 cm., and weighs 25·00 grams; while in 225 cm. fish the organ, which measured 70·00 cm., weighed 156·00 grams. These facts, especially the great size and weight of the organ in large skate (about 7 feet in length), do not seem to point to the skate's organ being in process of degeneration; more especially as the increase in size is not accompanied by any histological changes of a retrogressive nature, the largest organ examined being apparently as perfect as that of *Torpedo* and *Gymnotus*.

In discussing the organ from a physiological point of view, reference is made to the investigations of Sanderson and Gotch, and it is pointed out that, when the electric plate is taken as the unit, the value per square millimetre of the single plate of the skate is in all probability equal to, if not greater than, that of the torpedo.

The structures of the organs of the skate and torpedo are compared at length, and it is shown that in the case of the torpedo all the non-essential structures are absent, while the all-essential part, the electric layer or plate, closely resembles the corresponding layer or plate in the skate, the electric layer of *R. circularis* being especially like that of the torpedo.

In considering the modifications of the electric organ in the skate genus, it is shown that in all the British species, with the exception of *R. radiata*, *R. circularis*, and *R. fullonica*, the elements are in the form of discs. In the three exceptions the elements are more or less cup-shaped. In *R. radiata*, as described in a former paper, they are in the form of thick-walled shallow cups. The electric plate, apparently a greatly enlarged motor plate, lines the cup, which throughout resembles an ordinary striated muscle. In *R. circularis*, a more specialised member of the group, the electric elements are larger and better developed. The cups are deep and well moulded, and the electric layer is even more complex than in *R. batis;* at least, it more closely resembles the electric layer of the torpedo. Further, the cups are invested by a thick nucleated cortex, from which a number of delicate short processes project—the first appearance of the long prongs found in *R. batis*. In *R. fullonica* the electric elements stand nearly midway between the only partially transformed muscular fibres of *R. radiata* and the complex discs of *R. batis*. The cups in *R. fullonica* are less deep than in *R. circularis;* and while the electric and striated layers appear to be all but identical in the two species, the cortex is decidedly more like that of *R. batis*. The short simple processes of *R. circularis* are represented in *R. fullonica* by processes, often complex, which, by projecting freely from the outer surface of the cup, give it an irregular villous appearance, and at once suggest the processes or prongs which are so characteristic of the alveolar layer of *R. batis*.

After giving a summary of his observations on the electric organ of the skate, the author concludes by pointing out that it is not yet possible to indicate by what method the electric organs of fishes have been produced.

Presents, February 25, 1892.

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APPENDIX.

SUMMARY OF THE SECOND AND THIRD CHARTERS.

EXTRACTED FROM WELD'S HISTORY OF THE ROYAL SOCIETY, VOL. II., PP. 494–521.

Second Charter, 1663.

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494. Incorporation and Corporate Name.

494. The King himself Founder and Patron.

495. Capacity to purchase; and to grant; and to sue and be sued; and to have a common Seal; (which they have Liberty to alter at pleasure.)

495. Grant of Arms, viz. Argent; in a Canton Dexter, the three Lions of England: and also of a Crest, and Supporters.

496. The Council shall consist of Twenty-one; (of whom, the President or his Deputy shall be always one.)

496. All other Persons who shall be received and admitted as Members, by the President and Council, or any eleven or more of them (of whom, &c.), or by two Thirds or more of those eleven or more *within* two Months; and at all Times *after* those two Months; by the President, Council, and *Fellows*, or by any twenty-one or more of them (of whom the President or his Deputy to be one), or by *two third* Parts or more of the said twenty-one or more; and shall be registered; shall be called *Fellows* of the said Royal Society, for Life, unless regularly amoved.

497. William Lord Viscount Brouncker, named to be the first President; to continue so till the next St. Andrew's Day, and till another (out of the Council) should be chosen and sworn. He himself to be first sworn in before the Lord Chancellor.

497. The President's Oath.

497. The first Council named. To continue till next St. Andrew's Day, and till others shall be elected and sworn, unless amoved for just Cause; having first taken, before the President, the like Oath as he took, mutatis mutandis.

499. The President, Council, and Fellows, or any nine or more of them (of whom, &c.), may hold Assemblies at any Time or Place in London, or within ten Miles of it.

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499. The President, Council, and Fellows, or any thirty-one or more of them (of whom the President or his Deputy to be one), or the major part of such thirty-one or more, may upon every St. Andrew's Day, annually, elect one of the Council to be their President, who shall continue so, (if not dead or amoved,) till the next St. Andrew's Day, and till another shall be elected; having first been sworn in before the Council, or any seven or more of them.

500. On the Death or Amotion of a President, or if he quit, the Council or any eleven or more of them may meet to choose a President out of the Council: and the Person chosen by them, or the major part of them, being sworn, shall hold during the Residue of the Year, and until another shall be elected and sworn.

500. On the Death, Amotion, or quitting of any of the Council, (who are hereby made amoveable by the President and Council for sufficient Cause,) the President, Council, and Fellows, or any twenty-one or more of them (of whom, &c.), or the major part of such twenty-one or more, may supply the Vacancy from amongst the Fellows: And the Person or Persons elected shall hold, (being first sworn,) till the next St. Andrew's Day, and until another or others shall be elected.

501. On St. Andrew's Day, *Ten* of the Council (but no more) are to be changed by the President, Council, and Fellows, or any thirty-one or more of them (of whom the President or his Deputy always to be one,) or the major part of such thirty-one or more.

501. The President may appoint *One* out of the Council to be his Deputy; who may act as such in his Absence, unless the President make some other Deputy out of the Council.

502. The Deputy may, in the Absence of the President, do all Acts that he himself could do if present; but he must first be sworn before the Council, or seven or more of them.

502. The Society may have a Treasurer, two Secretaries, two or more Curators of Experiments, one Clerk or more, and two Sergeants at Mace to attend upon the President. All these are to be chosen and named by the President, Council, and Fellows, or any thirty-one or more of them (of whom the President or his Deputy to be one,) or by the major Part of such Thirty-one or more; And they must be sworn before the President or his Deputy, and the Council, or any seven or more of them.

503. The first Treasurer named; and also the two first Secretaries.

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503. On every St. Andrew's Day (unless it be Sunday, and then on the next Day,) the President, Council, and Fellows, or any Thirty-one or more of them (of whom, &c.), or the major Part of such Thirty-one or more, may elect proper Persons out of the Council to be Treasurer and Secretaries; who, after being sworn, are to hold their Offices till the following St. Andrew's Day.

504. If the Elections of President, Council, Treasurer, and Secretaries, or any of them, cannot conveniently be made or finished upon St. Andrew's Day, the President, Council, and Fellows, or any Thirty-one or more of them (of whom, &c.), or the major Part of such Thirty-one or more, may appoint one or more other Day or Days till they shall be finished.

504. If any of the said Officers die, quit, or be amoved, the President, Council, and Fellows, or any Twenty-one or more of them (of whom the President or his Deputy to be one,) or the major Part of such Twenty-one or more, may elect others for the Residue of the Year, and till new ones shall be elected and sworn.

504. The President and Council (*every* Member of the Council being always duly summoned to extraordinary Meetings,) or any Nine or more of them (of whom the President or his Deputy to be one) may meet in London or within ten Miles of London; and they, or the major Part of them, may make Laws, Statutes, and Ordinances, and transact all Matters relating to the Management of the Society and its Affairs, and all their Acts shall be valid: but their Statutes must be reasonable, and not contrary to Law.

505. The President, Council, and Fellows, or any Twenty-one or more of them (of whom the President or his Deputy to be always one), or the major Part of such Twenty-one or more, may appoint one Printer or more, and one Engraver or more, and authorise them, by writing under the common Seal, and signed by the President, to print such Things (touching or concerning the Royal Society) as shall be given them in Charge by the President and Council, or any Seven or more of them (of whom the President or his Deputy to be one), or the major Part of such Seven or more. They must be first sworn before the President and Council, or Seven or more of them.

506. The President, Council, and Fellows, or any nine or more of them (of whom, &c.), or the major Part of such nine or more, shall have the same Right to demand and receive (by their Assignee or Assignees) the Bodies of executed

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Criminals, and to anatomize them, as the College of Physicians and the Company of Surgeons of London use or enjoy.

506. Licence is given to them or any nine (as last above,) or the major Part of them, to hold a Correspondence, on Philosophical, Mathematical, or Mechanical Subjects, with all sorts of Foreigners, by Letters signed by the President or his Deputy, in the Presence of the Council, or any Seven or more of them, and in the name of the Society.

507. Licence given to the President, Council, and Fellows, or to the President and Council, or the major Part of them, to build a College or Colleges in London, or within ten Miles of it.

507. If any abuses shall happen, or Dissensions arise, they shall be reformed and settled by the Earl of Clarendon (Lord Chancellor) alone, if living; and after his Death by the Archbishop of Canterbury, the Chancellor or Keeper of the Great Seal, the Treasurer, Privy Seal, Bishop of London, and two principal Secretaries, for the Time being, or any four or more of them.

508. General Clauses.

Third Charter, 1669.

509. Grant of Lands in Chelsey; Tenure; Rent; Exonerations, Acquittances, &c.

514. Recital of some Parts of the Second Charter. It takes Notice that several Powers, granted by *that* Charter, cannot be executed but by the President and Council or *seven* or *more* of them; by virtue of *that* Charter. This Charter directs that the President's Deputy shall continue in Office, *although* the President do appoint one or more others: And it gives him express Power to appoint *two* or *more* Deputies, out of the Council, at one and the same Time; who may, each of them, do the same Acts in his Absence, as he himself could do if present. But they must first be sworn before the Council, or *five* or more of them.

518. For the Future, the President, Council, and Fellows, or any nine of them (of whom the President or his Deputy to be always one), may hold their Assemblies anywhere *within the Kingdom of* ENGLAND.

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518. All Powers, &c., which could not be exercised heretofore but by the President and Council, or *seven* or more of them, may for the future be exercised by the President and Council, or any *five* or more of them.

518. For the future, the *President* may appoint one Printer or more, and one Engraver or more, and authorise him or them to print such Things (touching or concerning the Royal Society) as shall be given to him or them in Charge, by the President and Council, or any *five* or more of them (of whom the President or his Deputy to be one), or by the major Part of such five or more. They must be first sworn before the President and Council, or any *five* or more of them.

519. General confirmatory Clauses.

520. The President and Vice-Presidents are enjoined to take the Oath of *Obedience* and the Oath of *Supremacy*, before the Council, or *seven* or more of them, previously to their acting.

STATUTES OF THE ROYAL SOCIETY, 1891.

CHAPTER I.

Of the Election and Admission of Fellows.

I. No person shall be proposed, elected, or admitted a Fellow of the Society on the day of the Anniversary Meeting for electing the Council and Officers.

II. Every Fellow, previously to his proposing a person as a Candidate for Election, shall inform him of the Obligation to be subscribed, of the sum to be paid for admission money, and of the payments to be made to the Society, before he can be admitted a Fellow.

III. Every such Candidate shall be proposed and recommended by a certificate in writing signed by six or more Fellows, of whom three at least shall certify their recommendation from personal knowledge. The certificate shall specify the name, rank, profession, qualifications, and usual place of residence of the Candidate; and being delivered to one of the Secretaries, or to the Assistant Secretary, shall be registered, with the date of delivery, in a book to be kept for the purpose, and read at the next ordinary meeting; and, if so ordered, shall be suspended in some convenient place in the Apartments of the Society until the day of Election.

IV. Any one of Her Majesty's subjects who is a Prince of the Blood Royal may, nevertheless, be proposed at one of the Ordinary Meetings of the Society by any Fellow, and may be put to the vote for Election on the same day, provided public notice of such proposition shall have been given by the proposer at the preceding Meeting of the Society.

Any Member of Her Majesty's Privy Council may be proposed at any Ordinary Meeting by means of a certificate prepared in accordance with Statute III. of this Chapter, no distinction, however, being made between personal and general knowledge, and the fact of the Candidate being a Member of the Privy Council being alone stated as the qualification. Such certificate, on being allowed by the Society, shall be suspended in some convenient place in the apartments of the Society until the day on which a ballot is taken upon it. The date proposed for the ballot, which shall not be earlier than the third Ordinary Meeting after that at which the certificate is read, shall be announced at the head of the certificate.

V. At the first Ordinary Meeting of the Society in March, the names of all Candidates proposed subsequently to the first Meeting in March of the preceding year, including those whose certificates have been resuspended as hereinafter provided, shall be announced by the Secretary from a list arranged in alphabetical order, without reference to the dates of the certificates of the Candidates; and these certificates shall remain suspended until the day of Election.

VI. In the first week in April a list shall be printed, containing the names of all the Candidates so announced at the first Meeting in March, arranged in alphabetical order, without reference to the dates of the certificates, together with the names of the Fellows by whom each Candidate is proposed and recommended; and a copy of such list shall immediately thereafter be sent to every Ordinary Fellow.

VII. The Council shall select by ballot from such printed list of Candidates a number not exceeding fifteen, to be recommended to the Society for Election; but no such selection by the Council shall be valid unless eleven Members at least be present and vote, a majority deciding, or in the event of equality the President having a second or casting vote.

VIII. At the first Ordinary Meeting of the Society in May, the President shall read from the Chair the names of the Candidates whom the Council have selected as most eligible, arranged in alphabetical order; and after such Meeting, a circular letter shall be forthwith sent to every Fellow, naming the day and hour of Election, and inclosing a printed list of the selected Candidates, with space for such alterations as any Fellow may determine to make in pursuance of Statute X. of this Chapter.

IX. The election of Ordinary Fellows not included in the privi-

leged classes referred to in Statute IV. of this Chapter, shall take place on the first Thursday of June; unless the Council shall alter the day of Election to any other day in the month of June, in which case due notice of such alteration shall be given to every Ordinary Fellow.

X. On the day of Election two Scrutators shall be nominated by the President, with the approbation of the Society, to assist the Secretaries in examining the lists; and each Fellow present and voting, shall deliver to one of the Secretaries or Scrutators one of the printed lists mentioned in Statute VIII. of this Chapter, having erased the name of any Candidate or Candidates for whom he does not vote, and, if he shall have thought fit, having substituted or added the name of any other Candidate or Candidates contained in the printed list sent in pursuance of Statute VI. of this Chapter.

XI. One of the Secretaries shall take down the names of the Fellows who vote, and the Scrutators, after examining the lists with the Secretaries, shall report to the President the names of the Candidates who shall have been duly elected in compliance with the Charters, and the President shall announce those names from the Chair.

XII. Any Candidate announced at the first Ordinary Meeting of the Society in March, as aforesaid, who shall not have been elected, shall, if his proposers, or any one of them, so request in writing, continue a Candidate; his name shall be placed in alphabetical order with those of the new Candidates to be announced in March following, and his certificate shall be suspended along with those of the new Candidates. Any additional qualifications of such a Candidate may be set forth in a supplementary certificate to be signed by not fewer than six Fellows.

XIII. Every person who is elected a Fellow shall appear for his admission on or before the fourth Ordinary Meeting of the Society after the day of his election, or within such further time as shall, for some sufficient cause, be granted by the Council; otherwise his election shall be void.

XIV. The admission of any Fellow into the Society shall be at some Ordinary Meeting, in manner and form following, he having first made the payments required by the Statutes. Immediately after the reading of the Minutes has been concluded, he shall subscribe the Obligation in the Charter-book, and be introduced to the President, who, taking him by the hand, shall say these words: *I do, by the authority and in the name of the Royal Society of London, for improving natural knowledge, admit you a Fellow thereof.*

XV. The Election, the payments made previous to admission, and the admission of every person into the Society, with the time thereof, shall be recorded in the Journal-book.

XVI. No person shall be deemed a Fellow of the Society until he has made the payments required by the Statutes; nor shall he be entitled to vote at any Election or Meeting of the Society until he shall have been admitted in the manner and form above specified.

XVII. Persons may be elected into the Society, under the title of Foreign Members, who are neither natives nor inhabitants of Her Majesty's dominions, and shall be exempted from the operation of Chapters II. and III. of these Statutes; they shall be selected from among men of the greatest eminence for their scientific discoveries and attainments.

XVIII. The Council shall from time to time, as they shall see fit, put in nomination persons for Election as Foreign Members, not exceeding, with those already elected, the number of fifty.

XIX. A book shall be kept in which Members of the Council may enter the names of those men of science whom they suggest as Foreign Members; each entry shall be signed by the proposer and be accompanied by a short statement of the principal grounds on which the suggestion is made, and shall be valid for three years only.

XX. When vacancies are to be filled up, a list of the persons so entered shall be sent to each Member of the Council together with notice of the Meeting at which the list will be considered. At the Meeting thus appointed further entries may be made, and the claims of those men of science whose names have been duly entered in the book shall be considered, and a selection of names shall be made, from among which the Council, at a subsequent Meeting to be then appointed, may make nominations to the Society.

XXI. At the second Meeting the selection of the Candidates to be nominated shall be by ballot; when, if two-thirds of the Members of the Council present be in favour of the nomination of any Candidate, his name shall be proposed at the next Ordinary Meeting of the Society, and shall be put to the vote at the following Ordinary Meeting.

CHAPTER II.

Of the Obligation to be Subscribed.

Every person elected a Fellow of the Society shall, before his admission, subscribe the Obligation in the following words :—

We who have hereunto subscribed, do hereby promise each for himself, that we will endeavour to promote the good of the Royal Society of London, for improving natural knowledge, and to pursue the ends for which the same was founded; that we will be present at the Meetings of the Society, as often as conveniently we can, especially at the Anniversary Elections, and upon extraordinary occasions; and that we will observe the Statutes and Orders of the said Society. Provided, that whensoever

any of us shall signify to the President under his hand, that he desireth to withdraw from the Society, he shall be free from this Obligation for the future.

And if any person elected shall refuse to subscribe the said Obligation, the election of that person shall be void.

CHAPTER III.

Of the Payments to be made by the Fellows to the Society.

I. Every person elected a Fellow of the Society shall, before he is admitted, pay the sum of *ten pounds* for admission money, the sum of *four pounds* for the year of his Election, and the same sum annually in advance so long as he shall continue a Fellow of the Society. And if any such person shall refuse or fail to pay the said sums, he shall not be admitted, and his Election shall be void: except the said sums be remitted in whole, or in part, by special order of the Council. Provided always that, except in the case of Fellows elected under Statute IV. of Chapter I., the admission fee of each Fellow shall be paid out of the Fee Reduction Fund, and shall not be demanded of the Fellow; and that, except in the case of Fellows elected under Statute IV. of Chapter I., and Fellows elected before January, 1879, *one pound* of the annual contribution shall be paid out of the Fee Reduction Fund.

II. All who have or may become Fellows of the Society may at any time compound for their annual payments, by paying at once the sum of *sixty pounds.*

III. All Annual Contributions shall be considered to be due on the 25th day of March in each year. Every Fellow of the Society liable to an Annual Payment shall (previously to the 25th day of March in every year) bring or send the same to the Treasurer or the Assistant Secretary. And if any such Fellow, after notice sent by post to his usual address, in May, and again in September, shall fail to pay the same before the first day of October in each year, his name shall be suspended in the public Meeting-room of the Society as being in arrear, and shall continue so suspended until the sum due be paid. And if any such Fellow shall fail to pay his subscription on or before the first day of November in each year, no satisfactory reason having been assigned to the President and Council for such non-payment, he shall cease to be a Fellow of the Society. Provided, nevertheless, that on a solicitation for readmission being addressed to the President and Council by an individual so circumstanced, within the space of one year following St. Andrew's Day, the case of the individual so soliciting shall be stated by the President from the Chair, at one of the Ordinary Meetings of the Society, and the question of his readmission be put to the vote at the next Ordinary Meeting of the Society.

CHAPTER IV.

Of the Death or Recess of any Fellow.

The Death or Recess of any Fellow of the Society shall be recorded in the Journal-book of the Society, and the names of such persons announced from the Chair, at the Anniversary Meeting for electing the Council and Officers.

CHAPTER V.

Of the Causes and Form of Ejection.

I. If any Fellow of the Society shall contemptuously or contumaciously disobey the Statutes or Orders of the Society or Council; or shall, by speaking, writing, or printing, publicly defame the Society; or advisedly, maliciously, or dishonestly do anything to the damage, detriment, or dishonour thereof, he shall be ejected out of the Society.

II. Whensoever there shall appear to be cause for the ejection of any Fellow out of the Society, the subject shall be laid before the Council; and if a majority of the Council shall, after due deliberation, determine by ballot to propose to the Society the ejection of the said Fellow, the President shall in that case, at some Ordinary Meeting of the Society, announce from the Chair such determination of the Council; and at the Ordinary Meeting next after that at which the said announcement has been made, the Society shall proceed to determine the question; and on its appearing that two-thirds of the Members present have voted for the ejection of the said Fellow, the President shall proceed to cancel his name in the Register, and at the same time pronounce him ejected in these words:—

I do, by the authority and in the name of the Royal Society of London, for improving natural knowledge, declare A. B. to be now ejected, and no longer a Fellow thereof.

And the ejection of every such person shall be then recorded in the Journal-book of the Society; and his name, as ejected, be also read at the next Anniversary Meeting for Elections.

CHAPTER VI.

Of the Election of the Council and Officers.

I. At the two Ordinary Meetings of the Society next preceding the day of the Anniversary Election, the President shall give notice of the said Election; and declare how much it imports the good of the Society, that such persons may be chosen into the Council, as are

most likely to attend the Meetings and business of the Council, out of whom there may be made the best choice of a President and other Officers.

II. Every Fellow of the Society whose residence is known, shall have notice of the Anniversary Meeting for electing the Council and Officers for the year ensuing, by particular summons, which summons shall be sent to the place of residence of such Fellow, a week at the least before the day of Meeting; and shall be to this effect:—

> *These are to give notice that on the day of the Council and Officers of the* ROYAL SOCIETY *are to be elected for the year ensuing; at which Election your presence is expected, at of the clock in the precisely.*

III. The Council for the ensuing year, out of which shall be chosen the President, Treasurer, Principal Secretaries, and Foreign Secretary, shall consist of eleven Members of the existing Council, and of ten Fellows who are not Members of the existing Council.

IV. The President and Council shall, previous to the Anniversary Meeting, nominate, by ballot, eleven Members of the existing Council, and also ten Fellows, not Members of the existing Council, whom they recommend to the Society for Election into the Council for the ensuing year. The President and Council shall, also, in like manner, nominate by ballot, out of the proposed Council, the persons whom they recommend to the Society for election to the offices of President, Treasurer, Principal Secretaries, and Foreign Secretary for the ensuing year.

V. At the ordinary meeting of the Society preceding the Anniversary Meeting, the names of such persons so recommended for Election as Council and Officers for the ensuing year shall be announced from the Chair.

VI. Lists, with the names of the Fellows recommended by the President and Council, and having a blank column opposite for such alterations as any Fellow may wish to make, shall be prepared for the use of the Fellows, one week before the day of election.

VII. Two Scrutators shall be nominated by the President, with the approbation of the Society, to assist the Secretaries in examining the lists.

VIII. Each Fellow voting, shall deliver his list to one of the Secretaries or Scrutators; and the name of each Fellow who shall so deliver in his list shall be noted by one of the Secretaries.

IX. The Scrutators, after examining the lists with the Secretaries, shall report to the Society the names of those having the majority of votes for composing the Council, and filling the offices of President, Treasurer, Principal Secretaries, and Foreign Secretary; the names of which persons shall then be announced from the Chair.

X. For electing any Member of the Council, or any Officer to be elected by the Society, upon such vacancies as shall happen in the intervals of the Anniversary Elections, the summons for such Election, and the proceedings in it, shall be after the same manner as is directed for the Anniversary Election.

XI. Upon any vacancy of the President's place, occurring in the intervals of the Anniversary Elections, the Treasurer, or, in his absence, one of the Secretaries, shall cause the Council to be summoned for the Election of a new President: and the Council meeting thereupon in the usual place, or any eleven or more of them, shall proceed to the said Election, and not separate until the major part of them shall have agreed upon a new President.

CHAPTER VII.

Of the President.

I. The business of the President shall be to preside at all the meetings, and regulate all the debates, of the Society, Council, and Committees; to state and put questions both in the affirmative and negative, according to the sense and intention of the meetings; to call for reports and accounts from Committees, and others; to check irregularities, and to keep all persons to order; to summon all Meetings of the Council, and Committee of Papers; and to execute, or see to the execution of, the Statutes of the Society.

II. The President shall take precedence of every Fellow of the Society, at their ordinary place of meeting; and also in all other places, where any number of the Fellows meet as a Society, Council, or Committee.

III. In the absence of the President, one of the Vice-Presidents shall act as his deputy, and may do, in the absence of the President, the same acts as the President himself could do if present.

CHAPTER VIII.

Of the Treasurer and his Accounts.

I. The Treasurer, or some person appointed by him, shall receive for the use of the Society, all sums of money due or payable to the Society; and shall pay and disburse all sums due from or payable by the Society; and shall keep particular Accounts of all such receipts and payments.

II. Every sum of money payable on account of the Society, exceeding Ten Pounds, shall be paid only by order of the Council; but payments for rates or taxes, to any amount, may be made by

the Treasurer, without any specific order of the Council for that purpose.

III. All sums of money, which there shall not be present occasion for expending, or otherwise disposing of to the use of the Society, shall be laid out in such Government or other securities as shall be approved of and directed by the Council.

IV. The Treasurer shall keep a yearly account of all such Fellows of the Society as pay the sum appointed as the composition in lieu of annual payments; and also of those who make the annual payments: and in this account shall be noted the times up to which the annual payments have been made, and the arrears due from each Fellow.

V. The Treasurer shall also keep a book of Cheque Receipts for annual payments, to be filled up with the name of the Fellow paying, the sum paid, and the time for which payment is made; these Receipts to be signed by the Treasurer, or by the Assistant Secretary receiving the money on the Treasurer's behalf, who, upon the delivery of the Receipt to the Fellow paying, is to enter upon that part of the Cheque which is left in the Book, the above particulars, and also the day of payment.

VI. The Treasurer shall demand, or cause to be demanded, all arrears of annual payments, as soon as convenient after the first day of May.

VII. The Accounts of the Treasurer shall be audited annually, a short time preceding the Anniversary Elections, by a Committee consisting of three Members of the Council, of whom the President or one of the Secretaries to be one; and of three Fellows of the Society not Members of the Council, who are to be nominated by the President, with the consent of the major part of the Fellows present, given by ballot at one of the three next preceding weekly meetings; any one or more of the said three Members of the Council, together with any one or more of the said three Fellows, shall be a Quorum of the said Committee; the Members of the said Committee who are of the Council shall make their Report to the Council held next after such audit, on or before the Anniversary Election; and the Members of the said Committee who are not of the Council shall make their Report to the Society, upon the Meeting next before the Anniversary Election, or on the day of the said Election.

VIII. The Treasurer shall have the charge of the Title Deeds of the Society's Estates, the Policies of Insurance, and Securities.

IX. As soon after the Audit as may be, and before the Anniversary Meeting, the Treasurer shall cause an abstract of the Society's Accounts of the preceding year to be printed for the use of the Fellows.

CHAPTER IX.

Of the Secretaries.

I. The Secretaries, or one of them, shall have inspection over the Assistant Secretary; and shall give the Orders and Directions concerning the entering and writing of all minutes or matters in the Journal-books of the Society or Council, or any other Books of the Society; and also concerning any orders or other writings for the use and service of the Society.

II. The Secretaries, or one of them, shall attend all meetings of the Society, Council, and Committee of Papers; where, when the President has taken the Chair, one of the Secretaries shall read the minutes, orders, and entries of the preceding meeting; and shall afterwards take minutes of the business and orders of the present meeting, to be entered by the Assistant Secretary in the respective books to which they relate.

III. At the meetings of the Society, Lists of the Presents made from time to time to the Society shall be laid on the Table, by one of the Secretaries, for the inspection of the Fellows; and the Thanks of the Society to the Donors shall be proposed from the Chair previously to the reading of the first Paper. One of the Secretaries shall give notice of any Candidate who stands proposed for election into the Society at that meeting; and the Secretaries shall read Letters and Papers presented to the Society, in such manner as the President shall direct.

IV. The Secretaries shall draw up all letters to be written to any persons in the name of the Society or Council (to be read and approved of in some meeting of either respectively), except, for some particular cause or consideration, some other person be appointed by the Society or Council to draw up any such letter. They shall likewise have the charge (under the direction of the Committee of Papers) of printing the *Philosophical Transactions*, the *Proceedings*, and other publications of the Society.

V. The letters relating to the business of the Society, received during each Session, shall be arranged and kept in the apartments of the Society.

VI. The duty of the Secretary for Foreign Correspondence shall be to receive and answer all letters from foreign parts relating to the business of the Society, to return thanks for Presents from Foreigners made to the Society, and to forward to persons elected Foreign Members the Diplomas certifying their election into the Society.

CHAPTER X.

Of the Assistant Secretary.

I. The person who shall be chosen to the office of Assistant Secretary, shall either not be a Fellow of the Society, or, if a Fellow, shall cease to be so, upon his election to and acceptance of that office.

II. The appointment of a person to the office of Assistant Secretary shall be by the Council, to whom the Officer so appointed shall give security, at the discretion of the Council; and he shall reside in the Society's House.

III. The Assistant Secretary shall be paid for his services, according to the determination of the Council; and shall not, besides such payments, receive any perquisite or profit whatsoever without the express permission of the President and Council. He shall be subject to such Rules and Orders as shall from time to time be made or given by the President and Council; and he shall constantly be in attendance during all meetings of the Society, Council, and Committees.

IV. He shall enter all the Minutes in the several Journal-books, and make an Index to every such book: he shall lay before every Council their fair Minute-book: and before every Committee of Papers the Society's Journal-book, to show that the several entries are fairly made: and he shall have the care of the writing of all Summonses of the Society, Council, and Committees.

V. He shall, under the direction of the Secretaries, have the charge and custody of the Charter-book, Statute-book, Journal-books of the Society and Council, Register-books, and Letter-books, as also of all Papers and writings belonging to the Society; all which shall be kept in the House of the Society, that they may be in readiness to be produced at any meetings of the Society or Council, as the case may require, or as shall be ordered by the Society, Council, or President.

VI. He shall not suffer any person, not being a Fellow of the Society, to read any Journal-book, Record, or Writing, or any part thereof, belonging to the Society; nor give any copy thereof, nor in any way communicate anything contained therein, to any such person.

VII. He shall follow the directions which may be given him from time to time by the Treasurer in respect of that part of his duties which relates to the Accounts or Cash Transactions of the Society. He shall enter in a Book, to be provided by the Treasurer, all such sums as he may receive on account of the Society at the instant of receiving such sums; and for these sums, so entered by him, he shall be answerable, until he shall have paid them to the Treasurer.

VIII. He shall attend the Library at such hours as shall be appointed for him for the accommodation of such Fellows of the Society as shall come to read the printed books or manuscripts, and of any other person who shall be introduced by a Fellow, either personally or by letter.

IX. He shall mark, with the stamp of the Society, all books accepted or bought by the Society.

CHAPTER XI.

Of the Meetings of the Society.

I. The Session of the Society shall commence on the third Thursday in November, and end on the third Thursday in June.

II. The ordinary Meetings of the Society shall be on Thursdays weekly (excepting Christmas, Passion, Easter, and Whitsun weeks, and such other weeks at Christmas and Easter, in each year, as the Council may in the preceding year determine, and also Ascension Day), and shall begin at half-past Four o'clock in the Afternoon precisely.

III. No stranger shall be permitted to be present during the Meeting, unless by invitation of the President, or by his leave or order upon the recommendation of some Fellow.

IV. The business of the Society in their ordinary Meetings shall be to order, take account, consider, and discourse of philosophical experiments and observations; to read, hear, and discourse upon letters, reports, and other papers, containing philosophical matters; as also to view, and discourse upon, rarities of nature and art: and thereupon to consider, what may be deduced from them, or any of them; and how far they, or any of them, may be improved for use or discovery.*

V. No letter, report, or other paper shall be read at any ordinary Meeting unless it be communicated by a Fellow or Foreign Member; and it shall be the duty of each Fellow or Foreign Member to satisfy himself that any letter, report, or other paper which he may communicate, is suitable to be read before the Society.

VI. The President shall determine for each Meeting the communications which are to be read, and the order in which they are to be taken. Every communication duly received shall, unless otherwise determined by the Committee of Papers, as provided in Statute I. of Chapter XIII., be read by one of the Secretaries, either in whole or in part, the title being considered a part, at some convenient ordinary Meeting of the Society, the President having power to invite the author of any communication to give an oral exposi-

* This is the wording of the Statute as given in the Statutes of 1663.

tion in place of the reading of the communication by one of the Secretaries.

VII. At the ordinary Meetings nothing relating to the Statutes or management of the Society shall be brought forward or discussed.

VIII. The Anniversary Meeting for the election of the Council and Officers, and the Annual Meeting for the election of Fellows, shall take place at an hour to be determined by the Council.

CHAPTER XII.

Of Special General Meetings of the Society.

I. The President or Council may at any time call a Special General Meeting of the Society when it may appear to them to be necessary.

II. Any six Fellows may, by notice in writing, signed by them, and delivered to one of the Secretaries at an ordinary Meeting of the Society, require a Special General Meeting of the Society to be convened, for the purpose of considering and determining on the matters specified in such requisition, and the Council shall, within one week after such requisition shall have been so delivered, appoint a day for a Special General Meeting accordingly.

III. One week's notice of any Special General Meeting shall be given to each Fellow resident in the United Kingdom, and such notice shall state the object of such Meeting.

IV. At such Meeting no business shall be brought forward except what shall have been so notified.

CHAPTER XIII.

Of the Publication of Papers.

I. The Members of the Council for the time being, shall constitute and be a standing Committee, to be called "The Committee of Papers," to whom the consideration of the Publication of all Papers which have been communicated to the Society at their weekly meetings, shall from time to time be referred, and who, in the case of any paper which, though duly received, shall be submitted by the President for their consideration, shall decide whether it shall be read or no.

II. The Committee of Papers shall meet at such times as shall be appointed by the President; due and sufficient notice of such meeting having been previously sent to every Member of the Committee. The meetings shall be of two kinds: ordinary meetings, at which any business relating to the publication of papers may be transacted, and

interim meetings held between ordinary meetings, at which only such business, relating to the publication of papers, as in the opinion of the President is not likely to give rise to difference of opinion, shall be transacted; and the summons to each meeting shall state whether the meeting is to be an ordinary meeting, or an interim meeting.

III. At an ordinary meeting no less number than seven of the Members of the said Committee (of which number the President, or in his absence a Vice-President, shall always be one) shall be a *Quorum*, capable of acting in relation to the said Papers. At an interim meeting any five of the Members of the said Committee shall be a *Quorum*. The minutes of an interim meeting shall be read for confirmation at the next ordinary meeting, and the minutes of an ordinary meeting shall be read for confirmation at the next ordinary meeting, not at any interim meeting which may intervene.

IV. The majority of the said Committee, present at any meeting thereof, shall decide with regard to any paper communicated to the Society, whether it shall be published in part or in whole in the *Philosophical Transactions* or in the *Proceedings* of the Society, and shall determine what parts, if any and not the whole, shall be so published. They shall further have power to require as a condition of publication such modifications of the text or of the illustrations as may seem to them desirable.

V. In the case of a paper communicated to the Society, in reference to which the Fellow (or Foreign Member) communicating it, has expressed the wish that it may be published in the *Philosophical Transactions*, the Committee of Papers, by a majority of those present, shall refer such a paper to at least two persons who are knowing and well skilled in the particular branch of Science to which the said paper relates, and who shall separately report in writing (or if one or both of them happen to be for the time being Members of the said Committee, he or they may report orally) their opinion of the said paper, and in particular as to its fitness to appear in the *Philosophical Transactions*. Such reports of referees shall be considered as confidential communications.

In the case of a paper in reference to which no such wish has been expressed by the Fellow or Foreign Member communicating it, the Committee of Papers shall have power to refer in like manner the communication, or not, as they shall see fit.

VI. The decisions of the Committee of Papers shall be determined by the majority of votes of those present and voting, and the voting shall be open, unless the President shall direct that the voting shall be by ballot. In case of an equality of votes, the President shall have a second or casting vote.

The decisions of the Committee shall be duly entered in the Minute-book of the Committee.

VII. Once, at least, in every year, a proper portion of the Papers which have been communicated to the Society, and so ordered for publication by the Committee of Papers, shall be printed under the name and title of *Philosophical Transactions of the Royal Society of London;* and from time to time a proper portion of the papers which have been communicated to the Society, and which have been ordered for publication by the Committee of Papers, but not in the *Philosophical Transactions*, shall be printed, together with such other matter as the Council may direct, under the name and title of *Proceedings of the Royal Society of London.* The time and manner of printing the *Philosophical Transactions* and *Proceedings* shall be fixed and determined by the Council, as occasion shall require. A number of the copies of the *Philosophical Transactions* and of the *Proceedings* so printed, sufficient to supply the Fellows of the Society, shall be delivered to the Assistant Secretary, who shall enter in a book, to be provided for that purpose, the number of copies received by him, for which he shall be accountable to the Council for the time being.

VIII. The *Philosophical Transactions* and *Proceedings* shall be printed at the sole charge, and for the use and benefit, of the Society, and of the Fellows thereof; to the intent that each of the present Fellows, who actually contributes and pays towards the support of the Society, or who has compounded for such contribution, according to the rules and orders established in relation thereto, or who has for other particular reasons been exonerated and discharged from such contribution by order of the Council, may receive, *gratis* (but under proper limitations and restrictions), one copy of such of the *Philosophical Transactions* and of the *Proceedings* as shall be printed as aforesaid; and that all persons who shall hereafter be admitted Fellows shall, under the same conditions, receive, and be entitled to, the like benefit and advantage.

IX. The Assistant Secretary shall deliver, *gratis*, one of the said copies of the *Transactions* to every Fellow of the Society (except as hereinafter excepted) who shall demand the same, either in person, or by letter.

Provided always, that no Fellow whatsoever of the Society shall be entitled to demand or receive any such copy of the *Transactions*, whose election and payment of Admission fees and regular Contributions shall not have preceded the date of the time appointed for the delivery of the said *Transactions;* neither shall the Executor of any deceased Fellow receive a copy of the *Transactions* published after the death of such Fellow.

Provided also, that no Fellow of the Society shall receive, or be entitled to receive, *gratis*, any copy or copies of the *Transactions*, so printed as aforesaid, after five years shall have elapsed from the time of the Assistant Secretary's having begun to deliver out such copies

respectively; but his neglecting to demand them for so long a time shall be deemed a forfeiture and dereliction of his right thereto: unless the Council for the time being, upon being made acquainted with the reason of such delay, and having regard to the circumstances of the application, and the amount of stock in hand, shall *order* such copies as they may think fit to be so delivered.

X. The Assistant Secretary shall further cause to be distributed, *gratis*, to all the Fellows of the Society, by post or otherwise, copies of the *Proceedings*, as soon as may be convenient after their appearance.

XI. If the number of copies of *Transactions* and *Proceedings* so to be printed shall be greater than what will be requisite to supply each of the Fellows with one copy, such supernumerary copies shall be disposed of at such times, and in such manner, as the Council shall direct.

CHAPTER XIV.

Of the Books and Papers of the Society.

I. There shall be had and kept a Book, called the *Charter-book*, wherein shall be fairly written the copy of the Charters, all the Royal Grants on behalf of the Society, and the Obligation to be subscribed by the Fellows of the Society in their own hand-writing.

II. There shall be kept a Book, called the *Statute-book*, wherein shall be fairly written, or printed, all the Laws, Statutes, and Constitutions made, or to be made, concerning the government and regulating of the Society or Council; and also a Register of the Fellows of the Society, with the times of their Election and Admission.

III. There shall be kept *Journal-books* of the Society, and also of the Council, wherein shall be entered all the minutes, orders, and business of the Society and Council at their respective meetings; to which Journal-books any Fellow may have access at such times as the Library is open.

IV. A Book shall be kept, in which the title of each communication received, the date of its reception at the apartments of the Society, and the name of the Fellow or Foreign Member who communicates it, shall be duly entered in the order of its reception.

V. The original copy of every Paper received at the Society shall be considered the property of the Society, if there be no previous engagement with its author to the contrary; but any author may withdraw a paper which has been received but not read; or may, by leave of the Council, have a copy of his paper; and it shall be in the power of the Council, if they think fit, to return to any author such drawings or other illustrations accompanying any paper communicated by him or on his behalf, which he may ask in writing to be returned to him.

VI. All the Papers not withdrawn by leave of the Council, and read to the Society, shall be delivered to the Committee of Papers; and all Papers which have not been printed in the *Transactions* or *Proceedings* shall be preserved in the archives of the Society for future inspection; and shall never be lent out of the Society's House without Order of the Council.

VII. The Library shall be open to the Fellows every week-day (exclusive of Good Friday and Easter-eve, of Easter week, of a week at Whitsuntide, and of a week at Christmas), from 11 a.m. to 6 p.m., except on Saturdays, when it shall be open from Eleven in the morning to One in the afternoon; but during the months of August and September it shall be closed on week-days, other than Saturdays, at 4 p.m.

VIII. Any Fellow may have the loan of any of the printed Books of the Society, excepting such as the Council shall order not to be taken out of the Library; but he shall not be allowed to have in his possession more than ten volumes at a time. The loan of Manuscripts is exclusively vested in the President and Council.

IX. A List of all Books and Manuscripts borrowed from the Library of the Royal Society, and of the Fellows of the Society to whom they are lent, shall be kept in the Library.

X. All Books whatsoever belonging to the Society shall be returned at a time to be specified by the Council, in each year; and the Library shall be closed for one month after such time, or for such shorter periods as the Council may direct.

XI. The value of such Books in the possession of any Fellow as are not returned to the Library pursuant to the preceding Statute, shall be required to be paid by the person who has so detained them.

CHAPTER XV.

Of the Common Seal and Deeds.

I. The Common Seal of the Society shall be kept in a box, the key of which shall be kept in a sealed packet. When the Common Seal has to be used, this packet shall be opened by the President in Council; and at the Council meeting at which it is so opened, the Common Seal having been replaced in the box, and the box locked, the key shall again be enclosed in a packet, which shall be sealed by the President with his private seal. The box and sealed packet shall be kept at the Society's chambers in an iron safe.

II. Every Deed or writing, to which the Common Seal is to be affixed, shall be passed and sealed in Council.

CHAPTER XVI.

Of the Restraint of Dividends to Fellows.

The Society shall not, and by its Laws may not, make any Dividend, Gift, Division, or Bonus in Money unto or between any of its Members.

CHAPTER XVII.

Of the Making and Repealing of Laws.

I. For the making of any Law or Statute of the Royal Society, the draught thereof shall be read in Council, and put to the vote, on two several days of their meeting. The first day the question to be resolved by vote shall be to this effect, viz., "Whether the draught of the said Statute, then agreed upon, shall be read at another meeting?" The second day the question shall be to this effect, viz., "Whether the draught of the said Statute, then agreed upon, shall pass for a Law, or not?"

II. For the repealing of any Law or Statute, or any part thereof, the Repeal shall be proposed and voted in Council on two several days of their meeting. The first day the question to be resolved by Ballot shall be to this effect, viz., "Whether the Repeal of such a Statute, or such part thereof, shall be proposed at another meeting?" The second day the question shall be to this effect, viz., "Whether such a Statute, or such part thereof, shall be repealed or not?" And in case the said Repeal be agreed unto, the same shall be recorded in the Journal-book of the Council; and the Statute, or part of the Statute, repealed, shall be cancelled in the Statute-book.

THE END.

A NOTE ON THE HISTORY OF THE STATUTES OF THE SOCIETY.

By M. Foster, M.A., M.D., LL.D., Senior Secretary.

The following note was drawn up for the use of the Council of the Society while preparing the foregoing revised edition of the Statutes :—

The First Statutes.

Ann. 1663. The second Charter, amending the first granted in 1662, having been granted April 22nd, 1663, the Statutes were drawn up in that year. A copy of these is published in Weld's "History of the Royal Society."

The Statutes from 1663 to 1752.

During the succeeding ninety years changes were from time to time made in the Statutes; but no new version of the Statutes appears to have been drawn up until the year 1752.*

"The laws of the Royal Society, like those of other communities, were altered from time to time, until they appeared sufficient to embrace every contingency that might occur, while they held their meetings in Gresham College, which they continued to do for near the space of fifty years. But the arrangement of the Society's affairs being somewhat altered upon possessing a house of their own, it became necessary to make different establishments in many particulars and to alter and augment some of their Statutes. However, the greater part of them was still left in the original form, suited to the situation of the Society at Gresham College."—(Preface to Statutes, Edition of 1776.)

Between 1663 and 1752, the following seem to have been the most important changes.

The Election of Fellows.

Ann. 1663. In the original Statutes, Cap. VI., "Of the Election and Admission of Fellows," Sec. 1 provides that candidates be propounded at one meeting, and put to the vote at some other

* The British Museum contains a small 8vo. edition, dated 1728, but this appears to be a verbatim copy of the Statutes of 1663, except that Cap. VI., Sec. 7, begins with the words "The admission of," instead of "The election and admission of."

meeting at which twenty-one Fellows (as prescribed by Charter) are present; but that every one of his Majesty's subjects having the title and place of Baron, or any higher title and place, and every one of his Majesty's Privy Council, may be propounded and put to the vote the same day. And Sec. 3 of the same chapter provides that "the name of every person propounded as a Candidate, together with the name of the Fellow proposing, shall be entered in the Journal-book;" by which it appears that "propounding" by *one* Fellow was sufficient.

Ann. 1682. In 1682, however, the following was proposed on August 2, and passed on August 5:—

"The Statute for Election of Fellows having by long Experience been found insufficient for bringing in persons qualifyed for the ends of the Institution of the Royal Society, few ballotting in the negative and presuming the person to be well known to the Member that Proposeth the Candidate, it is thought requisite by the Councell to propose this Statute following,—

"Every person that would propose a Candidate shall first give in his Name to some of the Councell, that so in the next Councell it may be discoursed *vivâ voce* whether the person is known to be so qualified as in probability to be usefull to the Society. And if the Councell return no other Answer but that they desire further time to be acquainted with the gentleman proposed, the Proposer is to take that for an Answer. And if they are well assured that the Candidate may be usefull to the Society then the Candidate shall be proposed at the next meeting of the Society and ballotted according to the Statute in that behalf, and shall immediately sign the usual Bond and pay his admission mony upon his Admission."

(Neither the Statutes of 1663, nor the Edition of 1752, make any mention of the "Bond for the payment of the contribution;" the words first occur in the Edition of 1776, but the actual Bonds preserved in the Archives of the Society date from January 1, 1674, onwards.)

Ann. 1727. In 1727 (January 9th) the following Statute was passed, that of 1682 being apparently repealed:—

"Every Person to be Elected Fellow of the Society shall first at a Meeting of the Society be propounded as a Candidate to be approved by the Council, and shall be recommended by three members, one of which at least shall be a member of the Council, and one of them shall at the same Time mention and specify the qualifications of the said Candidate. And afterwards such Person shall at another meeting of the Society (whereat there shall be a competent Number for making Elections) be refered back from the Council if approved, and shall then be propounded and put to the Vote for Election, Saving and Excepting that it shall be free for every one of his Majesties Subjects who is a Peer or the Son of a Peer of Great Britain or Ireland, and for every one of his Majesties Privy Council of either of the said kingdoms to

be propounded by any single Person and to be put to the Vote for Election on the same Day, there being present a competent Number for making Elections."

Ann. 1730. This, however, was in turn, very soon, viz., in 1730, changed to the following form, all mention of Council being omitted from the Statute:—

"X. Every person to be elected a Fellow of the Royal Society, shall be propounded and recommended at a meeting of the Society by three or more Members; who shall then deliver to one of the Secretaries a paper, signed by themselves with their own names, specifying the name, addition, profession, occupation, and chief qualifications; the inventions, discoveries, works, writings, or other productions of the candidate for Election; as also notifying the usual place of his habitation.

"A fair copy of which paper, with the date of the day when delivered, shall be fixed up in the common meeting room of the Society at ten several ordinary meetings, before the said candidate shall be put to the ballot: Saving and excepting, that it shall be free for every one of his Majesty's subjects, who is a Peer or the Son of a Peer of Great Britain or Ireland, and for every one of his Majesty's Privy council of either of the said Kingdoms, and for every foreign Prince or Ambassador, to be propounded by any single person, and to be put to the ballot for Election on the same day, there being present a competent number for making Elections."

It appears in this form in the Edition of 1752 as Sec. 10 of Cap. VI.

The Admission of Fellows.

Ann. 1727. In 1727, also on January 9th, the two following Statutes were enacted:—

"II. Every Person who is a Foreigner and every one of his Majesties Subjects whose habitation or usual place of residence is at more than forty miles distance from London, shall be and be deemed as a Fellow of the Society immediately after he shall be Elected, and shall be registered in the Journal Book of the Society as such: Provided always, that no such person shall have liberty to Vote at any Election or meeting of the Society before he shall be qualified pursuant to the Statutes. And if he shall neglect so to qualify himself the first time he comes to London when he may be present at a meeting of the Society and can be admitted; his Election shall be declared Void, and his Name shall be cancelled in the Register.

"III. No Person shall be Proposed, Elected, or Admitted a Fellow of the Society upon St. Andrew's Day or the Day of the Anniversary meeting for Electing the Council and Officers."

These appear in the Edition of 1752 as Secs. 8 and 9 respectively of Cap. VI.

As far, then, as the election and admission of Fellows are concerned, no new Statutes were enacted in 1752; the Edition of that year simply adds to the Statutes of 1663 the two enacted in 1727 and the one enacted in 1730.

The Election of Council and Officers.

Ann. 1663. In the original Statutes, Cap. VII., "Of the Election of the Council and Officers" makes arrangements that the eleven members of the existing Council who are to be continued should first be determined, after that the ten new members, and finally the officers. The Statutes of 1752, reproduce the chapter in its original form of 12 sections, with the addition of Sec. 13, enacted **Ann. 1735.** in 1735, which provides that in order to lessen the tediousness of the election, Fellows may give in *at the same time* three lists—(1) of 11 old Members of Council to continue, (2) of 10 new Members, (3) of Officers.

The Philosophical Transactions.

But the most important changes introduced in 1752, those which probably led to the issue of the new version of the Statutes in that year, **Ann. 1663.** relate to the Philosophical Transactions. In the old Statutes, Cap. XIII., "Of the Printer to the Society," provides for the printing and binding of books, catalogues, and such other things by order of the Society or Council; there are no other provisions as **Ann. 1665.** to publications. The Philosophical Transactions were begun in 1665; but up to the 46th volume inclusive, published in 1749–50, "the printing of them was always, from time to time, the single act of the respective Secretaries" (Adv. to Philosophical Transactions, vol. 47), though with regard to the first number the Council (Minutes, March 1, 1664) ordered "that the Philosophical Transactions, to be composed by Mr. Oldenburg, be printed the first Munday of every month, if he have sufficient matter for it, and that that Tract be licensed by the Council of the Society, being first reviewed by some of the Members of the same. And that the President be desired, now to Licence the first papers thereof, being written in four sheets in folio, to be printed by John Martyn and James Allestree," and this practice of licensing was continued with reference to those papers read before the Society which were published in the Transactions.

Ann. 1752. In 1752 it was determined to place the Philosophical Transactions directly in the hands of the Council, and the Edition of the Statutes of 1752, while leaving Cap. XIII. intact, adds the following two new chapters, enacted March 26th of that year:—

Cap. XX. "Of the selecting of Papers laid before the Society, in order for Publication," establishes and lays down regulations for the "Committee of Papers." These regulations are almost verbatim the same as Secs. 1 to 4 of Cap. XIII., "Of the Publication of Papers," of the Statutes in force at the present time, except that the Quorum of the Committee of Papers is five, not seven, and a provision is contained that no entry in the Minute-book of the Committee is to be made of Papers "thought improper to be laid before the public."

In the Statute in its original form the Committee "shall be at liberty to call in to their assistance . . . any other members of the Society who are knowing and well skilled in any particular branch of Science that shall happen to be the subject-matter of any paper which shall be then to come under their deliberation," and almost the same words are retained in the Statutes at present in force. The custom of the Committee is now, and for a long time has been, to "call in to their assistance" two or more Fellows, by asking for written reports, and such Fellows so assisting are generally spoken of as "referees." The earliest mention which has been found in the Society's records of a

Ann. 1780.

paper being "referred" is on May 25th, 1780, when a paper by Mr. Ludlow was "referred" to Mr. Cavendish and Dr. Hutton. There does not appear to be a similar record until

Ann. 1831.

March 21st, 1831, when a paper by Prof. Davy was referred to Mr. Faraday. By 1832, however, the practice of referring papers seems to have become very common. For some time the name of the person (or persons) to whom the paper was referred is stated in the Minutes of the Committee of Papers, and in all these cases, including those just mentioned, the persons in question were members of the then Council. Very soon, however, the name was omitted, the entry being simply "referred." There seems to be no means of ascertaining when "referees" outside the Council were first had recourse to, or when the practice of written reports first began.

Cap. XXI. "Of the Manner of Publication of the Papers laid before the Society, and defraying the Expences thereof," provides for the printing and distribution of the Philosophical Transactions, and is to a large extent, even in its very words, the same as Secs. 5 to 9 of Cap. XIII. of the Statutes at present in force, the word "Clerk" being used where "Assistant Secretary" is now used.

Payments by Fellows.

In order to defray the additional expenses thus incurred by the publication and gratis distribution to the Fellows of the Philosophical Transactions, the "admission-money" is by Sec. 2 of Cap. XXI. raised from two guineas to five guineas. In Cap. III. of the Statutes of 1663,

"Of the Payments by the Fellows to the Society," the admission-money is fixed at forty shillings, and indeed, in the Edition of 1752, the same sum of forty shillings is retained in this Chapter, the error apparently escaping notice. The change from forty shillings to forty-two shillings (two guineas) seems to have taken place at some time in the interval.

The Statutes from 1752 to 1776.

In 1774 and 1775, the Council were engaged in considering the Statutes, and in 1776 published a new Edition, containing several important changes. An interesting preface to this Edition (from which a quotation is given above), explains that in spite of large changes in the practices of the Society, the Statutes had been kept as far as possible in their original form; and, indeed, the Statutes of 1752 differ from those of 1663 chiefly in the additions described above. In 1776, however, the Council determined to bring the Statutes into more strict conformity with the practice of the Society, and in consequence the Edition of 1776 differs widely from the two earlier versions.

Five whole chapters are omitted, viz., V,—Of Experiments, and the Reports thereof; XI, Of Curators by Office; XIII, Of the Printer to the Society; XIV, Of Operators to the Society; XVII, Of Benefactors; the 21 chapters of 1752 being thus reduced to 16. The preface explains how the changes in the Society had long rendered these Statutes unnecessary.

The order of the several chapters is largely altered, the new arrangement adopted being that which has on the whole been followed in subsequent editions, and is still maintained.

The Election of Fellows.

Ann. 1776. The regulations for the election of Fellows remain on the whole the same, save that it is precisely stated that twenty-one is "the competent number" for making an election, a majority of two-thirds being necessary, and in the Statute relating to what we now call the "privileged class," the words "Foreign Prince or Ambassador" are replaced by the words "Foreign Sovereign Prince, or the son of a Sovereign Prince, or an Ambassador to the Court of Great Britain."

Composition Fee.

In the Edition of 1752, as stated above, no mention is made of any "bond" or "composition fee," but in the next year, 1753 (June 7), the Statute, Cap. VI., Sec. 8, concerning Foreigners and persons residing more than forty miles from London, was repealed, and the following substituted:—

Ann. 1753.

"That no one of his Majesties subjects, or any other person residing in his Majesties Dominions, who shall be elected a Fellow of the Society, shall be deemed an actual Fellow thereof, nor shall the name of any such person be Registered in the Journal Book, or printed in the List of Fellows of the Society, until such Person shall have paid his admission Fee, and given the usual Bond, or paid the Sum of Twenty-one pounds for the use of the Society in lieu of contributions: But that upon such payment or giving Bond as aforesaid, it shall be lawful for the Society to give leave for the name of any such person so elected as aforesaid to be entered in the Journal Book, and printed in the list of Fellows of the Society: Provided always that no such person shall have liberty to Vote at any Election or Meeting of the Society, before he shall be duly admitted a Fellow thereof pursuant to the former Statute."

This is the first time that the Statutes contain any reference to a composition fee.

Ann. 1766. In 1766 (December 11) a Statute was passed increasing the composition fee from twenty to twenty-six guineas; and the Statute of 1753 just quoted re-appears, with some slight changes, in the Edition of 1776 as Sec. 8 of Cap. I., the "sum of twenty-one pounds" being altered into "the sum appointed," and this the Chapter on payments by Fellows states to be twenty-six guineas.

Foreign Members.

The Statutes of 1776 contain, what the Statutes of 1752 and 1663 do not, special regulations for Fellows "residing in foreign parts and not subjects of the British Dominions."

Ann. 1664. So early as 1664 (Ap. 13) a Statute was passed providing that persons "residing in Forraigne parts," who are elected
Ann. 1716. Fellows, should not pay fees; in 1716 a reference occurs to
Ann. 1737. Foreigners who are Fellows; and in 1737 a resolution of Council (which did not become a Statute) proposed that Foreigners resident in London might be on the Home List if they paid contributions. It would appear, therefore, in spite of no mention of the matter being made in 1752, that, from an early period, a distinction was made between Fellows who were Foreigners and others, and that the Fellows who were Foreigners did not, of necessity, pay contributions to the Society. In the Register of Fellows, however, at this date no distinction of any kind is made.

It was apparently soon felt that the Foreign Members were too numerous and in some cases not of sufficient distinction; for
Ann. 1761. in 1761 (March 19) the Council, in order to ensure that "no persons residing in Foreign parts, not being subjects of the Crown of Great Britain, be elected Fellows unless their Qualifica-

tions be very well known as well abroad as at home," enacted a Statute providing that in the case of such persons the certificate should be signed by at least "three Foreign Fellows," as well as at least "by three Fellows named in the Home List." And in 1765 (Dec. 19) on a proposal "to restrain the number of foreign members," it was resolved "that no foreigner be proposed for election that is not known to the learned world, by some publication or invention which may enable the Society to form a judgment of his merit, and that till the number of foreign members be reduced to eighty, not more than two shall be admitted in one year." A special mode of procedure in the election of foreigners as Fellows was, at the same time, resolved upon, providing for the election of two a year; and a subsequent resolution (Dec. 26) provides that Foreign Members paying contributions shall "have their names printed in an alphabetical List next after that of the Home Members, as Foreign Members* contributing towards the expenses of the Society," and so distinct from "other foreign members" "who do not contribute." On January 16 of the next year the limitation to eighty was withdrawn, and the above resolutions were then embodied in the form of Statutes. These at the same time provided that the new regulation should not extend to Foreign Princes or their sons, and gave permission to foreigners resident in Great Britain to become Fellows in the usual way, which permission was extended on Jan. 26, 1769, to foreigners who had been resident in Great Britain for the space of six months. Soon after, namely on June 10th, 1773, the word "Foreigner" appears in the "Register" for the first time, being placed after the names of Stehelin, Le Roy, and Le Duc; thenceforward it is used frequently.

Ann. 1765.

Ann. 1766.

Ann. 1769.

Ann. 1773.

In the Edition of 1776 these regulations, in a somewhat modified form, are introduced as part of Sec. 8 of Cap. I.; the limitation to the election of two a year is omitted, and the certificates, signed by at least three Fellows upon the Foreign List, and at least by three Fellows on the Home List, are directed to be suspended from the 30th November until the weekly Meeting on, or next after, the 30th May. Some years afterwards, however (March 8, 1787), this part of Sec. 8 was repealed, and a new Sec. 9 added, which provides a somewhat complex mode of procedure in the election, under the title of "Foreign Members,"† of persons "who are neither natives nor inhabitants of his Majesty's dominions." The number is limited to one hundred. Certificates signed by six or more Fellows are to be presented at some meeting between Easter and the Anniver-

Ann. 1776.

Ann. 1787.

* It may be remarked that in the early records of the Society the words "Member" and "Fellow" appear to be used indiscriminately.

† Foreign *Member* as distinguished from *Fellow*. In the edition of 1776 and thenceforward the term Member, as applied to an ordinary Fellow, is never used.

sary. At a meeting immediately before the following Easter a selection of candidates is to be made, and the candidates so selected are to be balloted for at the next meeting immediately after Easter. These regulations are not, however, to apply to Sovereign Foreign Princes or their Sons, or to such Foreigners resident in Great Britain as may desire to become Fellows in the usual way.

The Officers of the Society, the Clerk, Librarian, &c.

No changes are made in the Statutes of 1776 for the election of Council and Officers; but to meet the changes in the contributions there are changes in the regulations for the Treasurer. There are also changes in the duties of the Secretaries, chiefly in reference to the Clerk and to the publication of the Philosophical Transactions.

Cap. X. provides regulations for the qualifications, mode of election, duties and remunerations of the Clerk, the Librarian, the Keeper of the Repository, and the House-Keeper.

Ann. 1663. The Statutes of 1663 contain regulations for the Clerk, and prescribe clerkly duties for him; and the Society had at first neither House-keeper nor Librarian.

Ann. 1710. When in 1710 the Society moved to Crane Court, the office of House-Keeper was established; but the then Clerk was made House-Keeper. As the Library and Repository were increased the offices of Librarian and Keeper of the Repository were established; but both these offices were held by the Clerk, under supervision, during a certain period at all events, of Fellows chosen for that duty under the title of "Inspectors." But the Statutes of 1752 contain no regulations for these offices other than that of the Clerk, the Statutes concerning whom remain exactly the same as in 1663; and in spite of the special regulations present in the edition of 1776, it appears
Ann. 1776. that the Society had never more than one officer to carry out these several duties, and that he was called "the Clerk," until at a later period (1823) the office of Clerk was abolished,
Ann. 1823. and that of Assistant Secretary instituted.

The Ordinary Meetings of the Society.

Ann. 1776. In the edition of 1776, Cap. XI. "Of the Ordinary Meetings of the Society," Sec. 1 provides that the ordinary Meetings should be held on "Thursdays, beginning at 6 p.m., and continue about an hour, as usual, at the discretion of the President."
Ann. 1769. This Statute was passed in 1769.
Ann. 1663. The Statutes of 1663 (IV., Sec. 1) provide that the ordinary meetings should be held on "Wednesday, beginning about three of the clock in the afternoon, and continuing until

six, unless the major part of the Fellows present shall, for that time, resolve to rise sooner, or sit later." And the Statutes of

Ann. 1752.

1752 reproduce exactly the Statute (IV. Sec. 1) of 1663. Nevertheless, the records of the Society show that the day and hour of the ordinary meeting were more than once changed in the interval, as they have been since. The following shows the changes and their respective dates up to the present time:—

1663.	On Wednesdays, at 2 p.m.		
July 1, 1663,	changed to	Wednesday,	3 to 6 p.m.
Feb. 5, 1666	,,	Thursday	at 3 p.m.
April 10, 1672	,,	Wednesday.	
Oct. 30, 1674	,,	Thursday	at 3 p.m.
Dec. 8, 1690	,,	Wednesday	,, 4 ,,
March 1, 1710	,,	Thursday	,, 4 ,,
April 20, 1769	,,	Thursday	,, 6 ,,
June 15, 1780	,,	Thursday	,, 8 ,,
(?) 1831	,,	Thursday	,, 8.30 p.m.*
Feb. 19, 1880	,,	Thursday	,, 4.30 ,,

Ann. 1831.

The first Statute enacting that no meeting should be held on certain days or in certain weeks was passed in 1831; previously to that the Statutes simply said "upon Wednesday," or "upon Thursday." But the practice of having an Autumn recess was of much older date than this; moreover, the Journal Book shows that from the earliest times it was customary to hold no meetings on Ash Wednesday and certain other holy days, and that in particular no meeting was held on the anniversary of the death of Charles I. In 1661 the Journal Book omits the date, January 30, without remark, although a meeting was due upon that day. On January 30, 1666, the Minute appears, "This day being the Anniversary Fast-Day, there was no Meeting of the Society." In 1667, the entry is, "The Society met not, because of the solemne Fast." Similar entries occur in subsequent years, the last being on January 30, 1834. After this date the custom was omitted.

The Admission of Strangers to the Meetings of the Society.

Ann. 1752.

In the Statutes of 1752, any of his Majesty's subjects having the title and place of a Baron, or having any higher title or place, are permitted to be present at the Meetings of the Society, "with the allowance of the President;" other persons may attend "upon leave obtained of the President and Fellows present."

* Careful search has failed to show when this change was made, but it was probably about this time.

Ann. 1776. In 1776 the mention of titled persons is omitted, and the Statute simply provides for "strangers" being present.

Ann. 1784 Some years later, viz., in 1784, a new section was added to Cap. XI. as follows:—

"VI. That the meetings of the Society may not be wasted by unprofitable debates, contrary to the intent and meaning of the fifth section of this chapter, it is constituted, established, and ordained, that every motion or question, proposed to be ballotted for by the Society, shall be fairly transcribed on paper, and being signed by six or more Fellows of the Society, it shall be by them delivered to one of the Secretaries at a meeting of the Society; and shall thereupon be read immediately after the declaration of the Presents on the table; and after being marked by the Secretary with the date of the day when delivered, it shall be fixed up in the common Meeting-room of the Society at the next ordinary Meeting; and on the Meeting next following the same, it shall be put to the Ballot, unless those who have signed it agree to withdraw it.

"But nothing contained in this Statute is to be construed to extend to matters relative to elections, or the ordinary business of the Society."

The motions or questions proposed to be "ballotted for" must therefore have had reference to matters of science.

Publications, Records, and Library.

Ann. 1776. In Cap. XII., the quorum of the Committee of Papers is raised from five to seven, and the part of the Statute providing that there should be no entry of rejected papers is omitted.

Ann. 1776. In Cap. XIII., "Of the Manner of Publication of the Papers laid before the Society," the word "Librarian" is substituted for that of "Clerk"; also the period during which surplus copies not required by Fellows must remain before they are disposed of by the Council, is extended from one year (as in 1752) to five years.

Ann. 1776. Cap. XIV., "Of the Books and Papers of the Society," differs somewhat from the corresponding Cap. XVI., "Of the Books of the Society," in the Statutes of 1752. The copy of Statutes, the List of Benefactors, and the Register of Fellows is omitted from the Charter Book.* The Statute concerning the Register

* The Charter Book never did contain, as provided by the Statute, the Register of Fellows, but only their signatures. The Society possesses, however, a volume now called "The Register," which contains the names, with dates of election, of all the Fellows from the foundation of the Society up to the year 1875. Since that date the Register is continued in a second volume.

Books, containing accounts of observations, experiments, &c., and the Statute concerning the Book of Letters, are omitted.

Ann. 1776. A new Statute (Sec. V.) is introduced, to the effect that the original copy of every paper read at the Society shall be considered as the property of the Society; and another (Sec. VI.) provides for the care of the papers read. And, lastly, a new Statute (Sec. VII.) introduces, for the first time, into the Statutes regulations concerning the use of the Library. The Library is to be open Tuesdays and Thursdays, from 11 a.m. to 2 p.m., and Fellows may, by leave of the Society or of the Council, take out four volumes for six weeks. If these are printed books, the Fellow gives merely his note; if MSS., a bond of £50 for each.

The Statutes from 1776 to 1847.

The Statutes of 1819.

The next edition appears to be that of 1819; it is, however, merely a reprint of that of 1776, with the additions of Cap. I., Sec. 9, as to Foreign Members, and Cap. XI., Sec. 6, as to the conduct of ordinary meetings, mentioned above (p. 508 and p. 511).

The Statutes of 1823.

Foreign Members, limited to fifty, selected by Council. In the next edition—that of 1823—several important changes are introduced. The number of Foreign Members is limited to fifty; and "they are to be put in nomination as candidates at a meeting of the Council," instead of the previous complex procedure. The regulations for the election of the Council and officers are much simplified, but not materially altered.

Foreign Secretary. A new Statute, Cap. IX., Sec 4, institutes a new office, that of the "Secretary for Foreign Correspondence." Since 1719 the proceeds of the bequest of Mr. Robert Keck had been "bestowed on some one of the Fellows," appointed "to carry on a foreign correspondence," but the Fellow performing these duties was appointed by Council at their pleasure, and was styled Assistant to the Secretaries. The new Secretary for Foreign Correspondence was to rank with the two Principal Secretaries.

Assistant Secretary. The office of Clerk is abolished, and that of Assistant Secretary created. The old Statute relating to the Clerk is, in consequence, largely modified. The Assistant Secretary is made Librarian and Housekeeper, but all mention of the Keeper of the Repository disappears from the Statutes. The facilities for using the Library are increased. The annual contribution is raised from "a **Contributions raised.** shilling a week," or thirteen shillings a quarter, to "one pound a quarter," the admission fee from five guineas

to ten pounds, and the composition fee from twenty-six guineas to forty pounds.

The Statutes of 1831.

The edition of 1831* contains a few changes which are of no great moment, and chiefly refer to payments (Cap. III.), the "bond" being omitted.

In 1831 the Statutes relating to the Assistant Secretary were amended, the separate regulations for Librarian and Housekeeper being omitted; and, in 1835, the then existing Statute, Cap. I., Sec. 5 (enacted in 1831), that "no election for Fellows, or for Foreign Members, shall take place excepting on the first ordinary meetings of the Society in December, February, April, and June" was repealed.

The Statutes of 1840.

In the next edition, 1840, the most notable change concerns the election of Officers and Council. These are to be put in nomination by the President and Council, according to the plan at present in use. A new Chapter, "Of Special General Meetings of the Society" is added. The composition fee is raised to sixty pounds in the case of Fellows elected after Dec. 11, 1834, except such as have contributed papers to the Philosophical Transactions; the Statutes concerning publications are thrown into one Chapter; and some slight changes are made in the Statutes concerning the Treasurer and Secretaries. Cap. XI., "Of the ordinary Meetings of the Society," provides for the recess from the third Thursday in June to the third Thursday in November, and as mentioned above, for the omission of meetings on certain days.

With the important exception of those relating to the election of Fellows, the Statutes of this edition are very like those at present in force.

The Statutes of 1847.

Very soon after, however, viz., in 1846, a Committee of Council was appointed to consider the mode of Election of Fellows, with the result that in 1847 new Statutes were enacted, regulating the Election of fifteen Fellows annually, according to the plan at present in use. These Statutes mark an epoch in the history of the Society.

* One form of this Edition is simply a reprint of that of 1823, with an Appendix of amended Statutes.

The Changes from 1847 *to* 1888.

The most notable changes which have since then been enacted or proposed are as follows:—

On November 3rd, 1864, the repeal of the Statute relating to the admission of strangers to the meetings was moved, but negatived; and again, on March 21st, 1867, a proposal that the public be admitted to the Ordinary Meetings of the Society was negatived.

In 1865 the privileged class (Cap. I., Sec. 4) was extended to include *Foreign* Sovereign Princes and their sons.

In 1866 the practice of paying for a proportional part of the year was abolished, and the annual payment was made one in advance.

In 1871 a new Statute was enacted prohibiting the payment of dividends to Fellows.

On October 30th, 1873, upon a motion to assimilate the mode of election of the Privileged Class to that of Ordinary Fellows, to place in the hands of the Council the selection of such candidates, and to require "evidence of ascertained special power and disposition to forward the aims of the Society from exceptional, personal, or official advantages of position, or of great eminence in any branch of learning, instead of any qualification based only on accident of lineage or of political status," the Statute concerned was referred to the consideration of a Committee, and on April 23rd, 1874, the Statute in its existing form was enacted.

On December 17th of same year, 1874, a Committee was appointed to consider the election of candidates for Fellowship, which Committee presented on November 30th, 1875, a long report giving reasons why no changes should be made.

In 1878–9 changes were made in the payment of fees.

In 1879 the Statutes relating to Foreign Members were altered to their present form.

In 1880 (February 19th) the hour of meeting was changed from the evening to the afternoon.

In 1885 the time during which the Library is open to Fellows was extended.

In 1888 the Statute, Cap. XI., Sec. 2, was altered to admit of an Ordinary Meeting being held on the day of Election of Fellows, and Statute, Cap. XIII., Sec. 7, was altered to allow Fellows to receive their copies of the Philosophical Transactions upon a request in writing.

In drawing up the above note I have been greatly assisted by the Assistant Secretary.

Appendix.

Number of Fellows from 1700 *to* 1890.

From 1700 to 1739, the numbers can be ascertained for certain years only. From 1740 to 1820, the numbers at the decades only are given.

The numbers are exclusive of Foreign Members, and the figures indicate the number of Fellows alive on the Anniversary Meeting of the year.

1701	125
1702	131
1703	127
1704	136
1705	138
1706	146
1708	149
1710	148
1711	152
1712	158
1713	160
1714	165
1715	162
1716	159
1717	161
1720	195
1721	194
1724	222
1731	268
1734	272
1735	279
1736	282
1738	295
1740	301
1750	348
1760	344
1770	378
1780	471
1790	493
1800	529
1810	547
1820	649
1821	675
1822	686
1823	674
1824	678
1825	678
1826	693
1827	679
1828	673
1829	670
1830	691
1831	695
1832	692
1833	690
1834	715
1835	735
1836	737
1837	730
1838	734
1839	749
1840	751
1841	769
1842	762
1843	769
1844	762
1845	767
1846	779
1847	768
1848	751
1849	748
1850	736
1851	720
1852	707
1853	701
1854	688
1855	671
1856	661
1857	658
1858	647
1859	637
1860	621
1861	607
1862	606
1863	602
1864	599
1865	586
1866	572
1867	564
1868	548
1869	544
1870	544
1871	542
1872	535
1873	524
1874	525
1875	515
1876	511
1877	505
1878	501
1879	488
1880	486
1881	480
1882	477
1883	473
1884	468
1885	465
1886	464
1887	465
1888	469
1889	466
1890	463

LIST OF PORTRAITS AND BUSTS IN THE APARTMENTS OF THE ROYAL SOCIETY AT BURLINGTON HOUSE.

*** Where the entries after a name are incomplete, particulars are wanting.

Sub ject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift
1. Amici, Giovanni Batista	Photograph ..	..	Sir C. Wheatstone, F.R.S.	1876
2. Arnott, Neil, F.R.S. ..	Crayon drawing	Mrs. Carpenter ..	Mrs. Arnott ..	1874
3. Arundel, Thomas Howard, Earl of	Oil painting ..	T. Murray ..	Sir Isaac Newton, P.R.S.	
4. Aston, Francis, Sec. R.S.	Ditto	F. Kerseboom		
5. Bacon, Sir Francis, Lord Chancellor	Ditto	P. van Somer ..	Martin Folkes, Esq., P.R.S.	1754
6. Baily, Francis, F.R.S...	Mezzotinto engraving	T. Lupton, after T. Phillips, R.A.	Rev. R. Sheepshanks	1846
7. Banks, Sir Joseph, Bart., P.R.S.	Oil painting ..	T. Phillips, R.A. ..	Don Jose de Mendoza y Rios	1818
8. Ditto	Marble bust ..	Sir F. Chantrey, R.A.	Sir F. Chantrey, R.A.	1819
9. Barrow, Sir John, Bart., F.R.S.	Oil painting ..	S. Pearce	J. Barrow, Esq., F.R.S.	1866
10. Bavaria, Charles Theodore, Duke of	Ditto	..	Duke of Bavaria ..	1785
11. Birch, Thomas, D.D., F.R.S.	Ditto	J. Wills		
12. Boyle, Hon. Robert, F.R.S.	Ditto	F. Kerseboom ..	Executors of Mr. Boyle	1692
13. Ditto	Ditto	Sir G. Kneller ..	Sir C. Wheatstone, F.R.S.	1876
14. Ditto	Ditto			
15. Bradley, James, D.D., F.R.S.	Ditto	J. Richardson ..	Rev. — Peach ..	1790
16. Brahe, Tycho	Ditto	M. J. Mierevelt		
17. Brodie, Sir Benjamin C., Bart., P.R.S.	Ditto	A. Thompson, after G. F. Watts, R.A.	Sir B. C. Brodie, Bart.	1873
18. Ditto	Plaster bust ..	Original model of the bust by W. Behnes	Sir B. C. Brodie, Bart.	1867
19. Brouncker, Viscount, P.R.S.	Oil painting ..	Sir P. Lely ..	Viscount Brouncker	
20. Buchanan, George, F.R.S.	Ditto	F. Pourbus, Sen. ..	T. Povey, Esq.	
21. Buckland, Rev. William, F.R.S.	Mezzotinto engraving	S. Cousins, R.A., after T. Phillips, R.A.	Sir C. Wheatstone, F.R.S.	1876
22. Buissière, Paul, For. Mem. R.S.	Oil painting ..	T. Gainsborough (?)	Peter Buissière, Esq., F.R.S.	
23. Burney, Dr., F.R.S. ..	Plaster bust			
24. Burrow, Sir James P.R.S.	Oil painting ..	J. B. Van Loo ..	Sir J. Burrow ..	1777

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
25. Chandler, Samuel, D.D., F.R.S.	Oil painting ..	M. Chamberlain ..	Executors of Mr. John Chandler, F.R.S.	1781
26. Chardin, Sir John, F.R.S.	Ditto	..	G. Handford, Esq.	1887
27. Charles II., King, Founder and Patron	Ditto	Sir P. Lely		
28. Ditto	Marble bust ..	J. Nollekens ..	Ordered by the Council R.S.	1779
29. Children, John George, Sec. R.S.	Oil painting ..	S. Pearce (?)* ..	Dr. J. E. Gray, F.R.S.	1873
30. Clift, William, F.R.S...	Ditto	H. Schmidt ..	Mrs. Owen ..	1858
31. Colwall, Daniel, F.R.S.	Ditto	..	D. Colwall, Esq...	†
32. Combe, Taylor, Sec. R.S.	Ditto	Joseph (?) ‡ ..	Dr. J. E. Gray, F.R.S.	1873
33. Copernicus, Nicholas ..	Ditto (on panel)	Lorman of Berlin, from an original portrait (see 'Phil. Trans.,' vol. lxvii., p. 33)	Dr. Wolf	1776
34. Cuvier, Georges ..	Bronze bust ..	..	P. J. David, Esq.	
35. Dalton, John, F.R.S. ..	Oil painting ..	B. R. Faulkner ..	A Memorial Committee	1841
36. Darwin, Charles, F.R.S.	Etching ..	P. Rajon, after W. W. Ouless, R.A.		
37. Ditto	Photograph (small oval)	..	Major Darwin ..	1887
38. Ditto ..	Bronze medallion	Allan Wyon		
39. Darwin, Erasmus, F.R.S.	Medallion, in Wedgwood	..	J. Evans, Esq., Treas. R.S.	
40. Davy, Sir Humphry, Bart., P.R.S.	Oil painting ..	Sir T. Lawrence, P.R.A.	Lady Davy ..	1829
41. Ditto ..	Photograph of the statue at Penzance	..	W. J. Henwood, Esq., F.R.S.	1873
42. Ditto ..	Wax medallion	J. Tayler		
43. De la Beche, Sir Henry Thomas, F.R.S.	Mezzotinto engraving	W. Walker, after H. P. Bone	Sir C. Wheatstone, F.R.S.	1876
44. Derham, Rev. William, D.D., F.R.S.	Oil painting .	G. White	G. Scott, Esq.	†
45. Descartes, René ..	Ditto	F. Hals	Dr. Maty	1776

* The portrait of Mr. Children was long in his possession, and given to me by him when he left the British Museum. I have failed as yet in getting any clue as to the painter of the picture. I have an idea that it was Mr. Pearce, who afterwards painted the Arctic people. (Letter from the donor, Aug. 4, 1873.)

† Included in a list printed in 1834.

‡ The portrait of Mr. Combe was given to me by Mr. Charles Tooke, his nephew, the son of his sister and T. Tooke, Esq., the author of 'Prices.' Mr. Combe married the daughter of Dr. E. W. Gray, Sec. R.S., my uncle. I believe the portrait is by Joseph, who painted all the family. (Letter from donor, Aug., 1873.)

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
46. Dollond, John, F.R.S...	Oil painting ..	W. F. Witherington, R.A.	G. Dollond, Esq., F.R.S.	1842
47. Ditto ..	Marble bust ..	— Garland ..	G. Dollond, Esq., F.R.S.	1843
48. Euler, Leonard ..	Plaster medallion			
49. Evans, John, Treas. R.S.	Bronze medallion	..	John Evans, Esq.	1889
50. Evelyn, John, Sec. R.S.	Oil painting ..	F. Kerseboom (?)	Mrs. Evelyn ..	*
51. Fairbairn, Sir William, F.R.S.	Ditto	B. R. Faulkner ..	Sir W. Fairbairn	1874
52. Ditto ..	Marble bust ..	P. Park	T. Fairbairn, Esq.	1862
53. Falconer, Hugh, F.R.S.	Ditto	T. Butler	A Memorial Committee	1866
54. Faraday, Michael, F.R.S.	Oil painting ..	A. Blaikley (painted between 1851 and 1855)	J. P. Gassiot, Esq., F.R.S.	1873
55. Ditto ..	Mezzotinto engraving	S. Cousins, R.A., after H. W. Pickersgill, R.A.	J. P. Gassiot, Esq., F.R.S.	1876
56. Ditto ..	Lithograph ..	..	Sir C. Wheatstone, F.R.S.	1876
57. Ditto ..	Marble bust ..	M. Noble	H. Bence Jones, Esq., F.R.S.	1873
58. Ditto ..	Plaster bust ..	J. H. Foley, R.A.	Purchased by the Council, R.S.	1885
59. Flamsteed, Rev. John, F.R.S.	Oil painting ..	T. Gibson	John Belchier, Esq.	1785
60. Ditto ..	Ditto	T. Gibson (?)		*
61. Folkes, Martin, P.R.S.	Ditto	W. Hogarth ..	Martin Folkes, Esq.	
62. Ditto ..	Plaster bust ..	..	Earl Stanhope ..	1871
63. Fontenelle, Bernard le Bovier de, For. Mem. R.S.	Oil painting ..	H. Rigaud ..	Dr. Maty, F.R.S...	1776
64. Forbes, Edward, F.R.S.	Plaster bust ..	J. G. Lough ..	Miss Lough-Bishop	1889
65. Franklin, Benjamin, F.R.S.	Oil painting ..	..	Caleb Whitefoord, Esq.	1790
66. Ditto ..	Plaster bust ..	..	Earl Stanhope ..	1871
67. Franklin, Sir John, F.R.S.	Lithograph ..	J. H. Maguire, after Negelin	Sir C. Wheatstone, F.R.S.	1876
68. Gale, Thomas, D.D., Sec. R.S.	Oil painting ..	J. Riley (?)		*
69. Galileo Galilei	Ditto	After J. Sustermans	Purchased	
70. Gassendi, Pierre ..	Ditto	..	Dr. Paget, F.R.S.	

* Included in a list printed in 1834.

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
71. George III., King, Patron	Marble bust ..	J. Nollekens ..	Ordered by the Council R.S.	1773
72. Gilbert, Davies, P.R.S.	Oil painting ..	T. Phillips, R.A. ..	Davies Gilbert, Esq.	1834
73. Ditto	Marble bust ..	R. Westmacott, R.A.	The Baroness Basset	1844
74. Graham, Thomas, F.R.S.	Mezzotinto engraving	J. Faed, after J. G. Gilbert	Sir C. Wheatstone, F.R.S.	1876
75. Gray, Edward Whittaker, Sec. R.S.	Oil painting ..	Sir A. Calcott, R.A.	Sir A. Calcott ..	1830
76. Gray, John Edward, F.R.S.	Ditto	Mrs. Carpenter .	The Botanical Society	1859
77. Haak, Theodore, F.R.S.	Ditto	J. Richardson		
78. Haller, Albert von, For. Mem. R.S.	Ditto	C. von Stoppelaer	Dr. Sharpey, F.R.S.	1877
79. Halley, Edmund, Sec. R.S.	Ditto	M. Dahl (?)	..	*
80. Ditto	Ditto	T. Murray (?)	..	*
81. Harvey, William, M.D.	Ditto	De Reyn	Dr. Mappletorf	*
82. Herschel, Sir John, Bart., F.R.S.	Oil painting ..	C. A. Jensen ..	John Evans, Esq., Treas. R.S.	1877
83. Hey, William, F.R.S...	Plaster bust ..	(Chantrey executed a marble bust from this plaster)	Rev. J. B. Reade, F.R.S.	1864
84. Hobbes, Thomas ..	Oil painting ..	W. Dobson† ..	Dr. Paget, F.R.S. (?)	*
85. Ditto	Ditto	After W. Dobson	..	*
86. Holland, Sir Henry, F.R.S.	Lithograph ..	..	Sir C. Wheatstone, F.R.S.	1876
87. Holman, Lieut. James, F.R.S.	Oil painting ..	G. Chinnery ..	Bequeathed by Lieut. Holman	1858
88. Home, Sir Everard, Bart., P.R.S.	Ditto	T. Phillips, R.A. ..	Sir E. Home, Bart.	
89. Hood, Thomas	Plaster bust ..	E. Davis	E. Davis, Esq. ..	1867
90. Hooker, Sir Joseph Dalton, P.R.S.	Oil painting ..	Hon. J. Collier ..	From sixty-eight Fellows, R.S.	1881
91. Humboldt, F. H. Alexander von, For. Mem. R.S.	Bronze statuette			
92. Hunter, John, F.R.S.	Oil painting ..	R. Home‡ . ..	Sir E. Home, Bart.	1850
93. Huxham, John, M.D., F.R.S.	Ditto	T. Rennel ..	J. C. Huxham, Esq., F.R.S.	
94. Huxley, Thomas Henry, P.R.S.	Etching (remarque proof)	L. Flemeng, after Hon. J. Collier	Fine Art Society ..	1885
95. Joule, James Prescott, F.R.S.	Oil painting ..	Hon. J. Collier ..	From a number of Fellows, R.S.	1883

* Included in a list printed in 1834.

† See Aubrey's 'Letters written by Eminent Persons,' vol. II, Part 2, p. 682, where he mentions portrait of Hobbes by J. B. Gaspars as presented to the Society.

‡ The dog in this picture is mentioned in 'Phil. Trans.,' vol. LXX, p. 257.

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
96. Jurin, James, M.D., Sec. R.S.	Oil painting ..	..	Rev. W. A. Totton	1868
97. Laplace, Pierre Simon, For. Mem. R.S.	Plaster bust			
98. Leibnitz, Gottfried Wilhelm, For. Mem. R.S.	Oil painting ..	..	Dr. Wilson ..	1883
99. Liebig, Justus von. For. Mem. R.S.	Photograph ..	..	Sir C. Wheatstone, F.R.S.	1876
100. Locke, John, F.R.S. ..	Oil painting ..	After Sir G. Kneller	J. Belchier, Esq. ..	1785
101. Lyell, Sir Charles, F.R.S.	Marble bust ..	W. Theed, after J. Gibson, R.A.	Leonard Lyell, Esq.	1878
102. Macclesfield, Earl of, P.R.S.	Oil painting ..	T. Hudson (?) ..	Earl of Macclesfield	1754
103. M'Culloch, John, M.D., F.R.S.	Ditto	B. R. Faulkner ..	Bequeathed by Mrs. M'Culloch	
104. Malpighi, Marcello, For. Mem. R.S.	Ditto	A. M. Tobar ..	Signor Malpighi	
105. Mantell, Gideon Algernon, F.R.S.	Ditto	J. J. Masquerier ..	W. Mantell, Esq. ..	1859
106. Maskelyne, Nevil, D.D., F.R.S.	Ditto	A. Vanderburgh ..	Mrs. Mervin Storey	
107. Moivre, Abraham de, F.R.S.	Ditto	J. Highmore ..	E. Wortley Montague, Esq.	
108. Moll, ——	Lithograph ..	H. W. Couwenberg	Sir C. Wheatstone, F.R.S.	1876
109. More, Henry, D.D., F.R.S.	Oil painting ..	Sir P. Lely ..	Dr. Paget, F.R.S.	
110. Murchison, Sir Roderick Impey, F.R.S.	Mezzotinto engraving	W. Walker, after W. H. Pickersgill, R.A.	Sir C. Wheatstone, F.R.S.	1876
111. Newton, Sir Isaac, P.R.S.	Oil painting ..	C. Jervas	Sir I. Newton	
112. Ditto	Ditto	J. Vanderbank* ..	C. B. Vignoles, Esq., F.R.S.	1841
113. Ditto .. .	Ditto	J. Vanderbank ..	Martin Folkes, Esq., P.R.S.	
114. Ditto	Mezzotinto engraving	J. Faber, after Vanderbank	R. Mallet, Esq. ..	1882
115. Ditto	Ditto	After Vanderbank	Rev. J. A. Edleston	1851
116. Ditto	Steel engraving	T. O. Barlow, R.A., after Sir G. Kneller	Dr. S. Crompton ..	1866
117. Ditto	Pencil drawing (signed D. L. Marchant)			
118. Ditto	Lithograph ..	— Baldrey, after L. F. Roubiliac's statue at Trinity College, Cambridge	Rev. C. Turnor, F.R.S.	1850
119. Ditto	Marble bust ..	L. F. Roubiliac		

* Painted the year before Newton died.

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
120. Newton, Sir Isaac, P.R.S.	Plaster statuette	W. Theed	J. Winter, Esq. ..	1858
121. Ditto	Ditto	H. J. Jones, after L. F. Roubiliac's statue at Trinity College, Cambridge		
122. Northampton, Spencer J. A. Compton, Marquess of, P.R.S.	Oil painting ..	T. Phillips, R.A. ..	Marquess of Northampton	
123. Oersted, Jens Christian, For. Mem. R.S.	Plaster bust ..	Bissen, of Copenhagen	Miss Harmer ..	1864
124. Oldenburg, Henry, Sec. R.S.	Oil painting ..	J. van Cleef ..	Purchased	
125. Paget, Sir James, Bart., F.R.S.	Steel engraving	T. O. Barlow, R.A., after J. E. Millais, R.A.	T. O. Barlow, Esq.	1875
126. Paget, Thomas, D.D.. .	Ditto	Mary Beale (?)		*
127. Peacock, George, Dean of Ely, F.R.S.	Oil painting ..	D. Y. Blakiston ..	A Committee of Subscribers	1860
128. Pepys, Samuel, P.R.S.	Ditto	Sir G. Kneller ..	S. Pepys, Esq.	
129. Pirogoff, ——.. ..	Photograph			
130. Price, Richard, D.D., F.R.S.	Oil painting ..	B. West, P.R.A. ..	Bequeathed by A. Morgan, Esq., F.R.S.	1876
131. Priestley, Joseph, F.R.S.	Photograph (from a portrait)	..	Sir C. Wheatstone, F.R.S.	1876
132. Pringle, Sir John, F.R.S.	Oil painting ..	Sir J. Reynolds, P.R.A.	Sir J. Pringle ..	1777
133. Ramsden, Jesse, F.R.S.	Ditto	R. Home	Sir E. Home, Bart.	1850
134. Rennell, James, Major, F.R.S.	Wax relief ..	— Hagbolt ..	Sir J. D. Hooker, F.R.S.	1890
135. Ditto	Porcelain medallion			
136. Ronalds, Sir Francis, F.R.S.	Plaster bust ..	E. Davis	S. Carter, Esq. ..	1871
137. Ross, Sir James Clark, R.N., F.R.S.	Lithograph ..	After Negelen ..	Lieut.-Col. Sabine	1846
138. Rosse, William Parsons, Earl of, F.R.S.	Oil painting ..	J. Catterson Smith	Earl of Rosse ..	1860
139. Sabine, General Sir Edward, P.R.S.	Ditto	S. Pearce	Mrs. Sabine ..	1866
140. Ditto	Marble bust ..	J. Durham ..	P. J. Gassiot, Esq., F.R.S.	1860
141. Schelling, Friedrich W. J. von	Lithograph ..	..	T. Handley, Esq.. .	1846
142. Schumacher, Heinrich Christian, For. Mem. R.S.	Oil painting ..	H. Wolf	H. Wolf, Esq.	

* Included in a list printed in 1834.

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
143. Sedgwick, Rev. Adam, F.R.S.	Mezzotinto engraving	S. Cousins, after T. Phillips, R.A.	Sir C. Wheatstone, F.R.S.	1876
144. Sloane, Sir Hans, Bart., P.R.S.	Oil painting ..	Sir G. Kneller ..	Sir Hans Sloane	
145. Smeaton, John, F.R.S.	Ditto	Mather Brown ..	A. Aubert, Esq., F.R.S.	*
146. Ditto	Ditto	J. Richardson ..	Ditto	*
147. Smith, Henry John Stephen, F.R.S.	Marble bust ..	J. E. Boehm, R.A. (a Replica)	A Committee of Subscribers	1885
148. Somers, John, Lord Chancellor, P.R.S.	Oil painting ..	Sir G. Kneller ..	Sir J. Jekyll	
149. Somerville, Mrs. ..	Marble bust ..	Sir F. Chantrey, R.A.	H.R.H. the Duke of Sussex and other subscribers	1842
150. Southwell, Sir Robert, P.R.S.	Oil painting ..	Sir G. Kneller ..	Sir R. Southwell	
151. Spelman, Sir Henry ..	Ditto	D. Mytens		
152. Spottiswoode, William, P.R.S.	Oil painting ..	Hon. J. Collier ..	A Committee of Subscribers	
153. Spratt, Thomas, Bishop of Rochester, F.R.S.	Wood engraving	M. V. Gucht, after Sir P. Lely	Dr. Eldridge Spratt	1880
154. Stephenson, Robert, F.R.S.	Steel engraving	F. Holl, after G. Richmond	Institution of Civil Engineers	1861
155. Stokes, Sir George Gabriel, Bart., P.R.S.	Oil painting ..	H. Herkomer, R.A.	Fellows of the Royal Society	1891
156. Sturm, John Christopher	Ditto	Heyman Dullaert	T. Haak, Esq., F.R.S.	
157. Sussex, H.R.H. the Duke of, P.R.S.	Ditto	T. Phillips, R.A. ..	The Duke of Sussex	
158. Taylor, Brook, Sec. R.S.	Ditto	A. Ramsay ..	Sir W. Young, Bart., F.R.S.	
159. Ditto	Autotype, after an original picture in the possession of Lady Young	..	Prof. A. G. Greenhill, F.R.S.	1889
160. Viviani, Vincentio, For. Mem. R.S.	Oil painting ..	..	Dr. Wilson ..	1883
161. Waller, Richard, Sec. R.S.	Ditto	T. Murray ..	R. Waller, Esq. ..	1711
162. Wallis, John, D.D., F.R.S.	Ditto	G. Soest	Mrs. Wallis	
163. Watson, Sir William, M.D., F.R.S.	Ditto	L. F. Abbot ..	Sir W. Watson	
164. Watt, James, F.R.S. ..	Marble bust ..	J. Hofferman, after Sir F. Chantrey, R.A.	— Watt, Esq. ..	1843
165. Wheatstone, Sir Charles, F.R.S.	Oil painting ..	C. Martin	Sir C. Wheatstone	1876
166. Wilkins, John, Bishop of Chester, Sec. R.S.	Ditto	Mary Beale		*
167. Williamson, Sir Joseph, P.R.S.	Ditto	Sir G. Kneller ..	Sir J. Williamson	

* Included in a list printed in 1834.

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
168. Wollaston, William Hyde, M.D., P.R.S.	Oil painting ..	J. Jackson, R.A. ..	Family of Dr. Wollaston	
169. Worcester, Edward Somerset, 2nd Marquess of	Steel engraving	W. Faithorne ..	H. Dircks, Esq. ..	1864
170. Wren, Sir Christopher, P.R.S.	Oil painting ..	Sir P. Lely (? Sir G. Kneller)	S. Wren, Esq. ..	*
171. Young, Thomas, M.D., F.R.S.	Ditto	H. P. Briggs, R.A., after Sir T. Lawrence, P.R.A.	Hudson Gurney, Esq.	1842

* Included in a list printed in 1834.

Miscellaneous.

Subject.	Description.	Painter, Engraver, or Sculptor.	Donor.	Date of gift.
1. Representing a Deputation from the Council of the Royal Society consisting of the President (Lord Wrottesley), Mr. Grove, and Mr. Gassiot to Mr. Faraday to urge him to accept the Presidentship, May, 1857	Oil painting ..	E. Armitage, R.A.	J. P. Gassiot, Esq. F.R.S.	1873
2. Manor House, Woolsthorpe, the birthplace of Sir Isaac Newton	Ditto	..	Rev. C. Turnor, F.R.S.	
3. Ditto, from another point of view	Ditto	..	Rev. C. Turnor, F.R.S.	
4. Village Church, Woolsthorpe	Ditto	..	Rev. C. Turnor, F.R.S.	
5. The President, Treasurer, the two Honorary and Assistant Secretaries of the Royal Society	Photograph			
6. Corona of the Sun, Eclipse of 1883	Original pencil sketches	W. H. Wesley ..	W. H. Wesley, Esq.	
7. The Moon	Enlarged photograph	..	W. De la Rue, Esq., F.R.S.	1864
8. Eruption of Krakatoa, May, 1883	Coloured sketch after a photograph taken during the eruption	..	The Krakatoa Committee of the Royal Society	

In addition to the series of framed engraved portraits, the Royal Society possesses a large collection arranged in portfolios; and a number of photographs of Fellows.

CATALOGUE OF THE MEDALS IN THE POSSESSION OF THE ROYAL SOCIETY.

*** The sizes of the Medals described in the Catalogue are given in inches and decimals.

1. **Amsterdam. Koninklijke Akademie van Wetenschappen.** Medal founded by M. Hoeufft, 1837, a gold example of which is awarded annually for a Latin poem. Poetry, holding in one hand a lyre, with the other places a laurel wreath upon the head of a poet who holds a scroll on which is inscribed CERTAMINA POETICA. Both are standing. *Legend.* CERTAMINA POESEOS LATINAE. *Exergue.* J. P. MENGER. F.
   Reverse. Within a laurel wreath, ACADEMIA REGIA DISCIPLINARVM NEDERLANDICA. *Exergue.* LEGATO IACOBI HENRICI HOEVFFT. 2·96. Æ.

2. **Baglivi, Giorgio, F.R.S.** Bust of Baglivi, *r.*, hair curly, in plain falling collar, doublet buttoned, and cloak. *Leg.* G. BAGLIVUS. MED . IN . ROM . ARCHIL . P . ET . SOC . REG . LOND . COLL . Behind S . V.
   Rev. A tripod encircled by a snake, between a mortar, retort and other implements of medicine, &c. *Leg.* VNAM . FACIEMVS . VTRAMQVE. *Ex.* MDCCIIII. 1·55. Æ.

3. **Baly, William.** Bust of Baly, *l.*, almost facing, open shirt. *Leg.* IN HONOREM GULIELMI BALY M.D. OB^{T}. 1861. Below, J. S. WYON SC.
   Rev. Representation of the façade of the Royal College of Physicians. Inscribed around, OB PHYSIOLOGIAM FELICITER EXCULTAM. Below, SIR R. SMIRKE R.A. ARCH^{T}.; J. S. & A. B. WYON SC. *Ex.* COLL . REG . MED . LOND. 2·28. Æ.

4. **Batavia. Bataviaasch Genootschap van Kunsten en Wetenschappen.** Medal struck in celebration of the centenary of the Society, 1778–1878. Within a wreath of tropical flowers, SOCIETAS . ART . SCIENT . BAT . IN . MEMORIAM . I . SAEC . FEL . CLAVSI . Below wreath, CH . WIENER . BRUXELLES. Inscribed within a border, A . D . VIII . K . MAI ; MDCCLXXVIII–MDCCCLXXVIII.
   Rev. A cocoa-nut tree (*Cocos nucifera*, Linn.) with outlines of Java mountains behind, and inscribed within a border, the motto, TEN NUTTE VAN 'T GEMEEN . BATAVIA'S GENOOTSCHAP. 2·89. Æ.

5. **Becquerel, Antoine César, For. Mem. R.S.** Head of Becquerel, *l.*, bare, hair short. *Leg.* ANTOINE CESAR BECQUEREL MEMBRE L'ACADÉMIE DES SCIENCES. Below, ALPHÉE DUBOIS.

Rev. Spaced on the field, OFFERT LE 13 AVRIL 1874 À L'ILLUSTRE DOYEN DES PHYSICIENS PAR SES CONFRÈRES PAR SES AMIS ET PAR SES ADMIRATEURS. 2·0. Æ.

6. **Beneden, Pierre J. van, For. Mem. R. S.** Arms of the University of Louvain with crest and supporters, and the motto, IN FIDE CONSTANS. Below, J. WIENER.

Rev. Inscription spaced on the field, CIVI SVO PRÆCLARO P. J. VAN BENEDEN PER ANNOS XL . IN UNIV . LOVAN . DOCENTI SCIENTIIS NATURALIBUS DOCTISSIMO CIVITAS MECHLINIENSIS DEDICAVIT A$^{D.}$ MDCCCLXXVII. 1·98. Æ.

7. ——— ——— ——— ——— —— Head of Van Beneden, *l.*, hair long, bearded. Below, ED . GEERTS . F.

Rev. Above, branches of laurel and palm intertwined with scroll inscribed PALÆONTOLOGIA . ANATOMIA . ZOOLOGIA. Inscription below, VIRO DOCTISSIMO ET CELEBERRIMO P. J. VAN BENEDEN PER DECEM JAM LUSTRA IN UNIVERSITATE CATHOLICA LOVANIENSI PROFESSORI . MDCCCXXXVI–MDCCCLXXXVI. 2·18. Æ.

8. **Berzelius, Jöns Jakob, For. Mem. R.S.** Bust of Berzelius, *r.*, hair short, bare. *Leg.* JACOBUS BERZELIUS . NAT . MDCCLXXIX . DEN . MDCCCXLVIII.

Rev. Inscribed around, APERIT AENIGMATA CONDITA LUSTRAT; right and left, C . G . OVARNSTRÖM . INV. P . H . LUNDGREN FEC. *Ex.* SOCIO LONGE NOBILISSIMO PER ANNOS XXX SECRETARIO ACAD . REG . SCIENT . SVEC. 2·22. Ʀ.

Bigsby, John Jeremiah, F.R.S. *See* London, Geological Society.

Black, Joseph. *See* Glasgow, University.

9. **Brahe, Tycho.** Bust of Brahe, *r.*, hair short, in richly embroidered doublet and mantle; round the neck a chain, to which a medallion portrait is attached. *Leg.* TYCHO BRAHÉ. Below, ROGAT . F.

Rev. Inscription, spaced on the field, NATUS ELSINBURGHI IN SCANIA AN . M.D.XLV . OBIIT AN . M.DC.I. SERIES NUMISMATICA UNIVERSALIS VIRORUM ILLUSTRIUM. M.D.CCC.XXV. DURAND EDIDIT. 1·64. Æ.

10. **Brussels. Académie Royale des Sciences et Belles-Lettres.** Medal celebrating the 100th anniversary of the foundation of the Academy by the Empress Maria Theresia. Bust of Maria Theresia, *l.*, hair in short curls, wearing bandeau decorated with pearls, drapery falling from the head, fastened at the breast with brooch; in low richly embroidered gown, and mantle fastened with jewel on the shoulder. *Leg.* IMP. MARIA THERESIA . ACAD . CONDIT. Below, B D U V.

Rev. Inscription, spaced on the field, ACADEMIA SCIENT . LIT . ET . ART . BELGICA AB . AVG . IMP . MARIA . THERESIA ANNO . MDCCLXXII . INSTITVTA A . GVILIELMO . I . REGE . AVGVSTO ANNO . MDCCCXVI . RESTITVTA A . LEOPOLDO . I . REGE . AVG . ANNO . MDCCCXLV . AVCTA FESTA . SAECVLARIA . AGIT ANNO . MDCCCLXXII. 2·0. Æ.

11. **Christiania. K. Norske Frederiks Universitet.** Within a

laurel wreath the inscription ACADEMIAE REGIAE NORV. FRIDERICIANAE SACRA SEMISECULARIA D. II SEPTBR. MDCCCLXI.

Rev. Inscribed around, EX HAUSTU OLYMPICO VALENTIOR. *Ex.* G. LOOS D. KULLRICH F. 1·67. GILT.

12. [**Christiania.**] **K. Norske Frederiks Universitet.** Medal founded 1872, on the occasion of the celebration of the union of Norway as one Kingdom one thousand years prior. A female figure representing Norway is seated to *l.*, helmeted, hair long; in right hand a spear, the left rests on the Norwegian shield. A mantle fastened at the throat with a brooch is thrown back, disclosing a vest of mail with waistbelt. Alongside the shield a stone, inscribed M[ILLE] ANNI. *Leg.* TEMPORI SVPERSTES. *Ex.* MDCCCLXXII. Below, MIDDELTHUN. INV: CONRADSEN. SCULP.

Rev. Within an olive wreath, REGNI NORVEGICI ANNVM MILLESIMVM PIA CELEBRAT VNIVERSITAS REGIA FREDERICIANA. 2·08. Æ.

13. ——— ——— **K. Norske Frederiks Universitet.** Medal founded 1873, on the occasion of the crowning of King Oscar II. Busts conjoined, *r.*, of Oscar II. of Sweden and Norway, and Sophia his Queen, both crowned. He wears a mantle fastened with a pin; she, a necklace. *Leg.* OSCAR II ET SOPHIA NOR. SUEC. REX ET REGINA MDCCCLXXIII. On truncation, G. LOOS D. On the rim, W. KULLRICH F.

Rev. Clio, seated, holding a scroll and pen. Around, an olive wreath. *Leg.* VETAT MORI. Below, U. R. F. [Universitas Regia Fredericiana.] On the rim, E. WEIGAND FEC. 1·68. Æ.

14. ——— ——— **K. Norske Frederiks Universitet.** Busts conjoined, *r.*, of Charles XV. of Sweden and Norway, and Louisa his Queen, both crowned. He wears a mantle fastened with a pin; she, a necklace. *Leg.* CAROLUS ET LOUISA NORV. SVEC. REX ET REGINA. Below, G. LOOS DIR. On the rim, SCHNITZSPANN FEC.

Rev. The goddess Athena, standing to *r.*, reading a scroll; on her breast the head of Medusa. To the right of the figure an owl flying to front. Inscription, within a border, VOVENS ET MEMOR. UNIVERSITAS REGIA FREDERICIANA. 1·68. Æ.

Clarke, Rev. William Branwhite, F.R.S. *See* Sydney, Royal Society of New South Wales.

15. **Combe, Taylor, F.R.S.** Head of Combe, *l.*, bare, hair short. On truncation, W. J. TAYLOR. F. Below, PISTRUCCI D.

Rev. Within a laurel wreath the inscription, TAYLOR COMBE M.A. SEC. ROY. SOC. DIRECT. SOC. ANT. KEEPER OF COINS & ANTIQUITIES BRITISH MUSEUM DIED 1826 AGED 52. 1·78. Æ.

Mr. Combe was Secretary of the Royal Society 1812–24. Pistrucci's original plaster model for this medal was presented to the British Museum by Dr. John Gray, F.R.S.

16. **Conduit, John, F.R.S.** Bust of Conduit, *r.*, hair short, neck

bare, in mantle, fastened with brooch on the shoulder. *Leg.* IOHANNES CONDUITT . REI . MONET : PRÆF : Below, TANNER . LONDINI . F.

Rev. Truth introduces Conduit to Hampden holding a staff surmounted by a cap of Liberty, a stork at his feet, and to Newton, seated, resting his hand on a slab, on which is a diagram of the planetary system. *Leg.* MEMORES FECERE MERENDO. *Ex.* M.DCC.XXXVII. 2·26. Æ.

Conduit succeeded Sir Isaac Newton, who was his uncle by marriage, as Master of the Mint.

17. **Copenhagen. Universitet.** Medal struck in celebration of the 400th anniversary. Busts conjoined, *r.*, of Christian I. (founder) and Christian IX., the one wearing a cap and falling collar, the other bare. *Leg.* CHRISTIANVS I . CHRISTIANVS IX . MDCCCLXXIX. On truncation, H. CONRADSEN.

Rev. Denmark seated, *l.*, laureate, and clad in loose draperies, clasps the extended hand of the goddess Athena standing, who holds an owl, on her breast the head of Medusa. The left hand of Denmark rests upon the Danish shield (three crowned lions, and nine hearts). Inscription, QVATTVOR EXEGIT SPERAT NOVA SAECVLA VIVAX. *Ex.* VNIVERSITAS HAVNIENSIS. 1·86. Æ.

18. **Copernicus, Nicholas.** Bust of Copernicus, *l.*, hair long, in coat and fur vest. *Leg.* NICOLAUS COPERNICUS. Below, PETIT F.

Rev. Inscription, spaced on the face, NATUS TORUNII IN PRUSSIA AN.M.CCCC.LXXIII . OBIIT AN.M.D.XLIII. SERIES NUMISMATICA UNIVERSALIS VIRORUM ILLUSTRIUM. M.DCCC.XVIII. DURAND EDIDIT. 1·6. Æ.

Copley Medal. *See* London, Royal Society.

19. **Czuczor, Gergely**, and **János Fogarasi.** Busts conjoined, *l.*, hair short, both bare. *Leg.* CZUCZOR GERGELY FOGARASI JÁNOS. Below, C. RADNITZKI.

Rev. Inscription, A MAGYAR NYELV SZÓTÁRA BEFÉJEZÉSÉNEK EMLÉKEŰL A MAGYAR TUDOMÁNYOS AKADEMIA MDCCCLXXIV. 1·68. GILT.

This medal was struck in commemoration of the completion of their great dictionary of the Hungarian language.

20. **Darwin, Charles, F.R.S.** Medallic Portrait. Cast. Bust of Darwin, *l.*, hair and beard long, crown of head bare. *Leg.* On sunk band, CHARLES DARWIN 1881. Below, A . L . [A. Legros.]

Rev. Plain. 4·5. Æ.

Darwin Medal. *See* London, Royal Society.

Davy Medal. *See* London, Royal Society.

21. **Descartes, René.** Bust of Descartes, *r.*, hair long, in plain falling collar and closely buttoned doublet. *Leg.* RENE DESCARTES. Below, GALLE F.

Rev. Inscription, spaced on the face, NÉ A LA HAYE EN

TOURAINE EN M.D.XCVI. MORT EN M.DC.L. GALERIE METALLIQUE DES GRANDS HOMMES FRANÇAIS. 1819. 1·6. Æ.

22. **Doncaster. Horticultural Society.** Bust of Linnæus, *r.*, hair short, in vest and cravat, with loose mantle. On the breast a sprig of *Linnæa borealis.* *Leg.* DONCASTER HORTICULTURAL SOCIETY, 1835. CAROLUS LINNAEUS. Below bust, J. B.
Rev. Within a wreath of flowers, the arms and crest of Doncaster. 2·1. Æ.

23. **Donders, Franz Cornelis, For. Mem. R.S.** Medal struck in honour of his Jubilee, celebrated at Utrecht in 1888. Head of Donders, *r.*, bare, hair short. *Leg.* FRANCISCVS CORNELIVS DONDERS. ·D . XXVII MAII A . MDCCCXVIII – MDCCCLXXXVIII. Below truncation, L. JÜNGER . D. J. P. M. MENGER . F.
Rev. Within an olive wreath, PER VARIAS GENTES ILLVSTRIS BATAVI ADMIRATIONE JVNCTI. Inscribed around, IN MEMORIAM DIEI QVO CONDITVM PIVM CORPVS IPSIVS NOMINE INSIGNE. Below, W. SCHAMMER. F. 2·6. Æ.

24. **Edinburgh. Royal Society.** The Keith Prize Medal. Bust of John Napier of Merchiston (the inventor of logarithms), *l.*, hair long, in ruff and close-fitting doublet. *Leg.* IOANNES NEPERUS DE MERCHISTON. Below, C. F. CARTER SCULP.
Rev. Within a laurel wreath, INGENII FELICITER EXCULTI PRÆMIUM KEITHIANUM. *Leg.* SOC . REG : EDIN : ADJUDICAVIT . 1·75. Æ.

Evans, John, F.R.S. *See* London, Numismatic Society.

Fogarasi, J. *See* Czuczor and Fogarasi.

25. **Folkes, Martin, P.R.S.** Bust of Folkes, *r.*, hair short, cap on head, in loose robe. *Leg.* MARTINUS FOLKES ARM^R. Below, JA. . ANT . DASSIER.
Rev. Within an ornamental compartment, SOCIETATIS REGALIS LONDINI SODALIS . M.DCC.XL. 2·15. Æ.
Folkes was President of the Royal Society for 11 years, having been elected in 1741.

26. **Freind, John, F.R.S.** Bust of Freind, *l.*, hair long, no drapery. *Leg.* IOANNES . FREIND . COLL . MED . LOND . ET . REG . S . S . On truncation, S V.
Rev. An ancient and a modern physician meeting and grasping right hands; between them, on the ground, are herbs, book, crucible, &c. *Leg.* MEDICINA . VETVS . ET . NOVA. *Ex.* VNAM FACIMVS VTRAMQVE. 2·26. Æ.

27. **Galileo Galilei.** Bust of Galileo, *r.*, hair short, bearded, wearing ruff and doublet. *Leg.* GALILÆUS GALILÆI. Below, GAYRARD F.
Rev. Inscription, spaced on the field, NATUS PISIS IN ITALIA . AN . M.D.LXIV . OBIIT AN . M.DC.XLII. SERIES NUMISMATICA UNIVERSALIS VIRORUM ILLUSTRIUM. M.DCCC.XVIII. DURAND EDIDIT. 1·6. Æ.

28. **Gauss, Carl Friedrich, For. Mem. R.S.** Bust of Gauss, *r.*, bare, hair long. *Leg.* CAROLVS FRIDERICVS GAVSS . NAT .

MDCCLXXVII APR . XXX OB . MDCCCLV FEB . XXIII. Below, BREHMER . F.

Rev. Within an ivy wreath, GEORGIVS V REX HANNOVERAE MATHEMATICORVM PRINCIPI. Inscribed around, ACADEMIAE SVAE GEORGIAE AVGVSTAE DECORI AETERNO. Below wreath, a star. 2·75. Æ.

29. [**Gauss, Carl Friedrich.**] Another copy. 2·75. Æ.

30. **Glasgow. University.** Bust of Joseph Black, *l.*, hair long, and tied behind, in coat and cravat. *Leg.* JOSEPHUS BLACK MDCCXXIII . MDCCXCIX. On truncation, N. MACPHAIL . SC.

Rev. Inscription, spaced on the field, IN ACADEM . GLASGUENS FACULTATE MEDICA DISCIPULUS INGENIO AC LABORE INSIGNIS PRŒMIUM HOCCE MERITO CONSECUTUS EST. 2·76. Æ.

A medical class medal of the University of Glasgow, where Dr. Black had been a professor.

31. **Gray, John Edward, F.R.S.**, and **Maria E. Gray.** Busts conjoined, *r.*, of Gray and Mrs. Gray. He, bare, hair short; she wears cap and dress with ribbon round the neck. Behind busts, in the field, I . E . & M . E . GRAY. Below, G. G. ADAMS . SC. 1863.

Rev. Within an olive wreath, TRUST IN THE LORD AND DO GOOD. 2·26. Æ.

32. **Haidinger, Wilhelm, For. Mem. R.S.** Head of Haidinger, *r.*, hair short. *Leg.* WILHELM HAIDINGER. Below. K. LANGE.

Rev. In relief, the Eastern hemisphere, around which are the signs of the zodiac. *Leg.* Inscribed within a border, NIE ERMÜDET STILLE STEHEN . MDCCCLVI. 2·52. Æ.

33. **Halley, Edmund, F.R.S.** Bust of Halley, *r.*, hair long, in loose mantle trimmed with fur. *Leg.* EDMUNDUS HALLEY . A . DASSIER . F.

Rev. Within an ornamental border, at the top part a festoon of flowers under a winged cherub, ASTRONOMUS REGIS MAGNÆ BRITANNIÆ. MDCCXLIV. 2·15. Æ.

Halley was Secretary of the Royal Society, 1713–21.

34. **Hansteen, Christopher, For. Mem. R.S.** Bust of Hansteen, *l.*, bare, hair short. *Leg.* CHRISTOPHORO HANSTEEN. Below, B. BERGSLIEN. F.

Rev. Within a wreath of oak and olive, the inscription, SPLENDET IN ORBE DECUS; above, a star. Inscribed around, SOLENNIA SEMISECULARIA GRATULATUR . UNIV : REG : FRED : MDCCCLVI. 1·5. Æ.

35. **Heidelberg. Universität.** Medal in celebration of the 500th anniversary of the University. Bust of Frederick of Baden, Protector of the University, *l.*, bearded, in uniform, with decoration, and loose mantle trimmed with ermine. *Leg.* FRIDERICVS . D . G . BADARVM . M . DVX . RECTOR . HEID . PERP. Below, SCHWENZER.

Rev. The Genius of Heidelberg standing and facing, her hands resting, on either side, on oval panels, bearing, on left, the bust of the Elector Rupert, *r.*, founder of the University,

1356; on right, bust of the Elector Charles Frederick, *l.*, who reconstituted the same in 1803. Jewelled scrolls carried from the base of each panel terminate with a laurel and palm branch. The central figure has long flowing hair, and wears loose drapery which leaves the arms bare; in left hand a laurel branch; her feet in sandals; at the girdle of her waist the arms of Baden. In the distance, in low relief, the Castle of Heidelberg. *Leg.* Inscribed within a border, VNIVERSITAS . HEIDELBERGENSIS . A . RVPERTO . CONDITA . A . CAROLO . FRIDERICO . INSTAVRATA. In the exergue, on a scrolled panel, SAECVLVM . SEXTVM . PIE . AVSPICATVR . A . D . MDCCCLXXXVI. Below, H . GÖTZ INV.; H . SCHWENZER FEC.

2·9. Æ.

36. **Hirn, Gustave Adolphe.** Medal, rectangular, struck in 1890, as a tribute of admiration for M. Hirn and his labours. Bust of Hirn, *r.*, hair long, in coat, collar, and cravat. *Upper leg.* GVSTAVE . ADOLPHE . HIRN. *Lower leg.* SES . COMPATRIOTES . SES . AMIS . SES . ADMIRATEVRS. Behind, NÉ AV . LOGELBACH . XXI . AOVT . MD.CCC.XV. Below, O . ROTY MDCCCLXXXIX.

Rev. Science, seated, *r.*, watches the flames of a fire burning upon an antique and raised altar, symbolical of thermodynamics. Her hair, with bandeau, gathered into a knot behind; she is clad in loose drapery, which leaves the arms bare; in left hand a scroll. At her feet an oak garland and a portfolio, whilst a balance recalls the applications of M. Hirn's researches on vapour. Behind her rise branches of laurel, among which the inscription SCIENTIA, on a band. In the distance the profile of the mountains of Logelbach with the ruins of the "Trois-Châteaux d'Eguisheim;" in the sky are stars, and Saturn. *Leg.* On a panel in the right-hand corner, AMICVS . PLATO SED . MAGIS . AMICA VERITAS. Below, O. ROTY.

2·42 × 1·8. Æ.

M. Hirn died Jan. 14, 1890, just before the issue of this medal.

Howard, John, F.R.S. *See* London, Statistical Society.

37. **Hume, Joseph, F.R.S.** Bust of Hume, *l.*, almost facing, hair short, in coat, collar, and cravat. *Leg.* JOS. HUME ESQ. M.P. F.R.S. Below, T.H. F.

Rev. A wreath of oak leaves above, underneath which inscription, OF CIVIL AND RELIGIOUS LIBERTY, THE VIRTUOUS AND ENLIGHTENED FRIEND: OF JUSTICE AND NATIONAL INTEGRITY, THE IMPARTIAL & UNDAUNTED DEFENDER. 1·52. Æ.

38. **Hunter, John, F.R.S.** Bust of Hunter, *r.*, bare; top of head, bare, hair around, curly. *Leg.* IOHANNES HVNTER. Below, B. WYON.

Rev. Within a laurel wreath, the armorial bearings of Yorkshire College, Leeds; below, on an ornamental scroll the motto ET AVGEBITVR SCIENTIA. Inscribed around, within a border, COLLEGIVM . COMITATVS . EBORACENCIS and SCHOLA MEDICINÆ. Roses separate the two groups of words. Below the shield, ALLAN WYON SC. 2·2. Æ.

39. **Hunter, William, F.R.S.** Bust of Hunter, *l.*, in tasselled cap, and shirt with collar open. *Leg.* GULIELMUS HUNTER MDCCXVIII . MDCCLXXXIII. On truncation of shoulder, N. MACPHAIL SC.
Rev. Inscription, spaced on the field, IN ACADEM . GLASGUENS . FACULTATE MEDICA DISCIPULUS INGENIO AC LABORE INSIGNIS PRŒMIUM HOCCE MERITO CONSECUTUS EST. 2·76. Æ.

Keith Medal. *See* Edinburgh, Royal Society.

40. **Lawrence, Sir William, Bart., F.R.S.** Head of Lawrence, *l.*, bare. *Leg.* GULIELMUS LAWRENCE, BARONETTUS . NAT : 1783 OB : 1867. Below, A. B. WYON.
Rev. Within an olive wreath, a shield bearing the arms of St. Bartholomew's Hospital. Inscribed around, within a border, S. BARTHOLOMÆI HOSP. ET COLL. INST. 1123. 1·52. Æ.

Lee, John, F.R.S. *See* London, Numismatic Society.

41. **Linnæus, Carolus.** Bust of Linnæus, *r.*, hair long, in vest and cravat, with mantle over the shoulders. On the breast of the coat a sprig of *Linnœa borealis*, and his decoration as Knight of the Polar Star. *Leg.* CAROLUS LINNAEUS ARCH . REG . EQV . AURATUS. Below, LIUNGBERGER.
Rev. Cybele standing, murally crowned, with lion crouching by her side; in her left hand a key, the right is upraised; she is surrounded by animals and plants, in the distance are clouds with flying birds. Three small butterflies are represented to the left of the figure. Inscription, DEAM LUCTUS ANGIT AMISSI. *Ex.* POST OBITUM UPSALIAE D . X . JAN . MDCCLXXVIII . REGE JUBENTE. 2·1. Æ.

42. **Locke, John.** Bust of Locke, *l.*, hair long, in shirt open at the collar, and loose mantle. *Leg.* JOANNES LOCK. Below, JAC . ROETTIERS.
Rev. Inscription, spaced on the face, MENS HABITAT MOLEM . VIRG . GEOR : M.DCC.LXXIV. 2·1. AR.

43. **London. Medal struck to commemorate the visit of H.M. Queen Victoria to the Corporation of London, Nov. 9, 1837.** Bust of Queen Victoria, *l.*, bare, wearing a diadem, hair bound with fillet and gathered into a knot behind. *Leg.* VICTORIA REGINA. On truncation, W. WYON . R.A.
Rev. A representation of the frontage of the Guildhall, with the Royal Standard floating above. *Exergal leg.* only, IN HONOUR OF HER MAJESTY'S VISIT TO THE CORPORATION OF LONDON 9TH NOV : 1837. 2·16. Æ.

44. ——— **City and Guilds of London Institute.** Siemens Medal. Head of Siemens, *l.*, bare, bearded, crown of head bare. *Leg.* SIR C. WILLIAM SIEMENS . F.R.S . D.C.L. IN MEMORIAM . BORN 1823 . DIED 1883. Below neck, ALLAN WYON.
Rev. Within an ivy wreath, FOR PROFICIENCY IN ELECTRICAL ENGINEERING. Inscribed around, THE CITY AND GUILDS OF LONDON INSTITUTE . CENTRAL INSTITUTION. Below wreath, A. WYON. 2·02. Æ.

45. ——— **Geological Society.** Bigsby Medal. Bust of Bigsby,

bare, *l.* *Leg.* J. J. BIGSBY M.D. F.R.S. BIENNIAL PRIZE MEDAL FOUNDED 1876. Below, A. B. WYON.

Rev. In the centre, a representation of an extinct species of echinoderm (*Agelacrinites Dicksoni*), and inscribed around, AGELACRINITES DICKSONI. FOUND . 1822 . CANADA. Below, J. S. & A. B. WYON. Beyond, within a border, AWARDED BY THE GEOLOGICAL SOCIETY OF LONDON FOR WORK OF GREAT MERIT. 1·78. Æ.

A gold example of this medal is awarded biennially by the Geological Society of London. The Royal Society possesses another specimen of nearly similar design but larger type; it was struck in bronze, and subsequently discarded for the smaller size in gold.

46. [**London.**] **Geological Society.** Bigsby Medal. *See* note above. 2·52. Æ.

47. ——— **King's College.** Siemens Medal. Head of Siemens, *l.*, bearded. *Leg.* CAR . GUL . SIEMENS PRAEMIUM IN ARTE METALLURGICA D . D . MDCCCLXXXII. Below truncation, J. S. & A. B. WYON.

Rev. The arms, crest, and supporters of King's College, London, with inscription above, COLL: REG: LOND: In the exergue on scrolls, SANCTE ET SAPIENTER. Below, A B . WYON . SC. 1·7. Æ.

48. ——— **King's College.** Todd Medal. Bust of Todd, *l.*, hair long, in coat, collar, and bow tie. *Leg.* ROBERT BENTLEY TODD M.D. F.R.S. DIED 30 JAN. 1860. Below, J. S. WYON SC.

Rev. The arms, crest, and supporters of King's College, London, with inscription, KINGS COLLEGE LONDON . FOR CLINICAL MEDICINE. On scrolls beneath arms, SANCTE ET SAPIENTER. Below, J. S. WYON SC. 3·0. Æ.

49. ——— **Numismatic Society.** Bust of John Lee, F.R.S. (first President), *r.*, drapery on neck. Below, STOTHARD . F.

Rev. Spaced on the field, NUMISMATIC SOCIETY OF LONDON FOUNDED DEC^R. XXII MDCCCXXXVIII. JOHN LEE L.L.D F.R.S: F.S.A: F.R.A.S PRESIDENT. 1·75. Æ.

50. ——— **Numismatic Society.** Jubilee Medal, 1887. Bust of John Evans, F.R.S., *r.*, hair short, in coat and collar. *Leg.* IOH . EVANS . D.C.L. S.R.S. PRAESIDI. On truncation, PINCHES . F.

Rev. Within an olive wreath, SIC / L / SIC / C Inscribed around, SOCIETAS NVMISM . LOND . ANNOS CONST . LI . MDCCCLXXXVII. 2·26. Æ.

51. ——————— Jubilee Medal. Another copy. 2·26. Æ.

52. ——— **Royal Astronomical Society.** Medal of the Royal Astronomical Society. Bust of Newton, *l.*, bare; behind, NEWTON *Leg.* ROYAL ASTRONOMICAL SOCIETY INST: MDCCCXX. Below, NUBEM PELLENTE MATHESI. On truncation, W. WYON . A R A . MINT.

Rev. Herschel's telescope. *Leg.* QUICQUID NITET NOTANDUM. (In the exergue of this specimen is inscribed, STRUCK BY PERMISSION OF THE COUNCIL FOR THE COLLECTION OF THE REV$^{D.}$ CHARLES TURNOR . 1840.) 1·9. AR.

This medal was ordered to be struck when the Society received its Royal Charter of Incorporation in 1831, it replacing an earlier type. The first impression was issued in 1834. It is struck in gold, and awarded annually or otherwise, as the Council determines.

53. [**London.**] **Royal Exchange.** Medal struck to commemorate the laying of the first stone of the Royal Exchange. Bust of Queen Victoria, *l.*, bare, wearing a diadem, hair bound with fillet, and gathered into a knot behind. *Leg.* VICTORIA D : G : BRITANNIARUM REGINA F : D : On truncation, W WYON . R A.

Rev. Inscription, spaced on the field, IN COMMEMORATION OF LAYING THE FIRST STONE OF THE NEW ROYAL EXCHANGE BY H : R : H : PRINCE ALBERT CONSORT OF H : M : QUEEN VICTORIA 17 JANUARY 1842 IN THE FIFTH YEAR OF HER REIGN. 1·78. Æ.

54. ——— **Royal Society.** Copley Medal. Athena, seated amidst emblems of her own attributes, and of the arts and sciences, holds out in the right hand a wreath; in her left arm is the Ephesian Artemis; on her breast the head of Medusa; near her the armorial shield of Sir Godfrey Copley. *Leg.* G . COPLEY BAR$^{T.}$ DIGNISSIMO. Below, T. [John Sigismund Tanner.]

Rev. The armorial shield of the Royal Society, with crest and supporters. *Leg.* SOCIETAS REG . LONDINI. *Ex.* On a band the motto NULLIUS IN VERBA. 1·7. Æ.

On the obverse of this specimen is inscribed, in the exergue, CAROLO LYELL EQ: 1858.

The Copley Medal, founded in 1736 under the will (1709) of Sir Godfrey Copley, Bart., F.R.S., is awarded annually for distinguished philosophical research, and irrespective of nationality. It is struck in gold.

55. —— —— —— —— —— Copley Medal. Another copy, but without exergal inscription. 1·7. Æ.

56. ——— **Royal Society.** Darwin Medal. Bust of Darwin, *l.*, hair and beard long, crown of head bare, in coat and collar. On truncation, ALLAN WYON SC.

Rev. Within a wreath, composed of the leaves and flowers of plants identified with Darwin's researches (*Ampelopsis*, *Drosera*, *Primula*, *Nepenthes*, *&c.*), the inscription CAROLVS DARWIN between the dates MDCCCIX and MDCCCLXXXII. Below wreath, ALLAN WYON. 2·25. Æ.

The Darwin Medal was founded in 1890, and is awarded biennially for work of distinction in the field in which Mr. Darwin himself laboured. It is struck in silver or bronze.

57. ——— **Royal Society.** Davy Medal. Bust of Sir Humphry Davy, *r.*, hair short, in coat, collar and cravat with frilled shirt. On truncation, A. B. JOY SC. N. MACPHAIL F.

Rev. Inscription, spaced on the face, THE ROYAL SOCIETY TO [recipient's name.] IN ACCORDANCE WITH THE WILL OF HUMPHRY DAVY WHO DEVOTED THE TESTIMONIAL PRESENTED TO HIM BY THE COALOWNERS OF THE TYNE AND WEAR TO THE ENCOURAGEMENT OF CHEMICAL RESEARCH. Below, under a line, the date. [The date on this medal is 1890.] 2·98. Æ.

The Davy Medal was founded in 1869 under the will of Dr. John Davy, F.R.S., a brother of Sir Humphry Davy, and is awarded annually for the most important discovery in chemistry made in Europe or Anglo-America. It is struck in gold.

58. **[London.] Royal Society.** Davy Medal. Another copy, inscribed ROBERT WILHELM BUNSEN : GUSTAVE ROBERT KIRCHHOFF. Dated 1877. 2·98. Æ.

59. ——— **Royal Society.** Royal Medal. Bust of Queen Victoria, *l.*, bare, wearing coronet, hair bound with fillet and gathered into a knot behind. *Leg.* VICTORIA REGINA SOC : REG : LOND : PATRONA . MDCCCXXXVIII. On truncation, W. WYON . R.A.

Rev. A representation of the statue of Sir Isaac Newton, by Roubiliac, in the Chapel of Trinity College, Cambridge. On either side of the statue are devices illustrative of Newton's discoveries. The diagram on the right is taken from the sixty-sixth proposition of the "Principia;" that on the left illustrates the solar system. *Leg.* REGINAE MVNIFICENTIA ARBITRIO SOCIETATIS. Below statue, NEWTON. 2·86. Æ.

Two Royal medals were founded by George IV., and are awarded annually for the two most important contributions to the advancement of Natural Knowledge published originally in the British dominions, within a period of not more than ten and not less than one year of the date of the award. They are struck in gold and in silver.

60. ——— **Royal Society.** Rumford Medal. A tripod, surmounted by a flame, with inscription around, NOSCERE QUÆ VIS ET CAUSA. Below, J . MILTON F.

Rev. Inscribed within an ornamental border of leaves, PRÆMIUM OPTIME MERENTI EX INSTITUTO BENJ . A RUMFORD S . R . I . COMITIS ADJUDICATUM A REG . SOC . LOND. 3·4. Æ.

The Rumford Medal was founded by Count Rumford in 1796, and is awarded biennially for the most important discoveries in heat or light during the preceding two years The medal is struck in gold and in silver.

This type was discontinued by order of the Council of the Society, Jan. 15, 1863, and on the recommendation of the Master of the Mint. *See* description below of medal now in use.

61. ——— —— Rumford Medal. Head of Rumford, *l.*, bare. *Leg.* BENIAMIN AB RVMFORD S . ROM . IMP . COMES INSTITVIT. Below, MDCCXCVI. On truncation, CH . WIENER.

Rev. Within a wreath of oak and laurel leaves bound with ribbons, OPTIME IN LVCIS CALORISQVE NATVRA EXQVIRENDA MERENTI ADIVDICAT SOC : REG : LOND : 3·4. Æ.

62. ——— —— Rumford Medal. Another copy. 3·4. Æ

63. **London. St. Thomas's Hospital.** Solly Medal. Head of Solly, *l.*, bare, hair short, crown of head bare. *Leg.* SAMUEL . SOLLY . F.R.S. Below, AFTER E . B . STEPHENS A.R.A . J . S . & A. B . WYON.

Rev. Inscribed around, IN MEMORY . OF . SAMUEL . SOLLY . F . R . S. SURGEON TO ST. THOMAS'S . HOSPITAL ✤ FOUNDED . A.D. 1873 ✤ Inside on the face, AWARDED FOR EXCELLENCE OF SURGICAL REPORTS TO [recipient's name]. 2·76. Æ.

64. ——— **Statistical Society.** Howard Medal. Bust of Howard, *l.*, hair long, and tied behind; in coat, collar, and cravat. *Leg.* JOHN HOWARD F.R.S. SHERIFF OF BEDFORD . 1773. Below, A. B. WYON.

Rev. A sheaf of corn, erect, with inscription, HOWARD . PRIZE . FOUNDED . 1873 . WILLIAM A. GUY M.B. F.R.S PRESIDENT. Inscribed around, within a border, STATISTICAL SOCIETY . ESTABLISHED . 1834. 3·0. Æ.

65. **Marlborough, Charles Spencer, Duke of, F.R.S.** Bust of the Duke, *r.*, in armour and riband across the breast. *Leg.* CAROLUS SPENCER. Below, J. A. DASSIER.

Rev. Inscription, DUX DE MARLBOROUGH. M.DCC.XLII. 2·16. Æ.

66. **Martius, Carl Friedrich Philipp von.** Bust of Martius, *l.*, bare, hair short. *Leg.* CAR . FR . PH . MARTIVS . Below, A. STANGER . F.

Rev. Within a border, spaced on the face, the inscription, VIRO IN BOTANICA PRINCIPI STVDIO FIDE CONSILIO SIBI PROBATISSIMO ACADEMIA R . BOICA D . LVB . MERITO TERTIO KALEND . APRIL . M.D.CCC.LXIIII. *Outer leg.*, above, CANDIDE ET FORTITER. Below, RERVM COGNOSCERE CAVSAS. 1·9. Æ.

67. **Modena. Società Italiana delle Scienze.** Medal in celebration of the centenary of the Society. An eagle upon her nest, with wings expanded, the rayed sun above. Below, INSENGA. *Leg.* within a border, SOCIETA' ITALIANA DELLE SCIENZE FONDATA NEL 1782.

Rev. Within a circle, the inscription, LA SOCIETA' ITALIANA DELLE SCIENZE NELL' AN . 1882 CENTENARIO DELLA FONDAZIONE. Without, a wreath of oak and laurel. 2·22. Æ.

68. **Moivre, Abraham de, F.R.S.** Bust of De Moivre, *r.*, hair long, in coat buttoned in front. *Leg.* ABRAHAMUS DE MOIVRE . Below, I . A. DASSIER .

Rev. Within an ornamental border, UTRIUSQUE SOCIETATIS REGALIS . LOND . ET . BEROL . SODALIS . M.DCC.XLI. 2·15. Æ.

69. **Montreal. McGill University.** Head of Sir Isaac Newton, *l.*, bare. *Leg.* SCIENTIIS . MATHEMATICIS . ET . PHYSICIS . FELICITER . EXCULTIS. Behind bust, NEWTON. Below, J. S. & A. B. WYON SC.

Rev. Above, the arms, crest, and motto of the Molson family. Within an olive wreath, ANNA · MOLSON DONAVIT 1864. Inscribed around, UNIVERSITAS MCGILL MONTE REGIO . IN DOMINO CONFIDO. 1·78. Æ.

70. **Montreal. McGill University.** Head of Watt, *r.*, hair short. Behind, in the field, JAMES WATT. *Leg.* PRESENTED AT M^CGILL UNIVERSITY . MONTREAL . PRIZE FOR APPLIED SCIENCES. Below truncation, ALLAN WYON . SC.

Rev. A wreath of maple and rose leaves, with thistles and roses. Inscribed around, IN MEMORY OF THE MEETING OF THE BRITISH ASSOCIATION AT MONTREAL. 1884. Below wreath, A. WYON. 1·78. Æ.

71. **Muratori, Ludovico Antonio.** Bust of Muratori, *r.*, hair long, in the cap and garb of a priest of the Romish Church. *Leg.* LODOVICO ANT . MURATORI. Below, F. SPERANZA.

Rev. Within a laurel wreath, the inscription, AL . PADRE DELLA STORIA . ITALIANA IL . MUNICIPIO DI . MODENA XXI . OCTOBRE MDCCCLXXII. 2·16. Æ.

72. **Newton, Sir Isaac, P.R.S.** Bust of Newton, *l.*, hair short, in shirt with open collar and mantle round the shoulders. *Leg.* ISAACVS . NEWTONVS. Below, I . C .

Rev. Science, with wings on her head. seated, *l.*, leans upon a table, and holds a diagram of the solar system. *Leg.* FELIX . COGNOSCERE . CAVSAS. *Ex.* M.DCC.XXVI. 2·04. AR.

73. ——— ———— —— Bust of Newton, three-quarters, *r.*, hair long, in shirt with open collar, and mantle around the shoulders. *Leg.* ISAACVS NEWTONIUS . Below, I. DASSIER . F.

Rev. A representation of Newton's monument in Westminster Abbey; on the base is inscribed, NAT . 1642 . M . 1726. 1·68. Æ.

74. ——— ———— —— Bust of Newton, three-quarters, *l.*, looking *r.*, hair long, in shirt with open collar, and mantle round the shoulders. *Leg.* ISAACUS NEWTONIUS.

Rev. A wreath of flowers enclosing the inscription, EQ . AUR . PHILOSOPHUS . OBIIT 31 . MART . 1727 . NATUS ANNOS 85 . 1·32. Æ.

75. ——— ———— —— Bust of Newton, *l.*, hair long, in shirt with open collar, and loose mantle. *Leg.* S^R ISAAC NEWTON.

Rev. A device of a caduceus, with cornucopiæ and laurel branch. Inscription, HALFPENNY. 1793. 1·12. Æ.

76. ——— ———— —— Another copy. Same as preceding, but smaller, and *rev.* without caduceus; the inscription, FARTHING. 1793. 0·88. Æ.

77. ——— ———— —— Bust of Newton, *l.*, hair long, in cravat and plain coat. *Leg.* ISAACUS NEWTONIUS. Below truncation, PETIT . F.

Rev. Inscription, spaced on the face, NATUS VOLSTROPII IN ANGLIA AN . M.DC.XLII . OBIIT AN . M.DCC.XXVII. *Ex* SERIES NUMISMATICA UNIVERSALIS VIRORUM ILLUSTRIUM. M.DCCC.XIX. DURAND EDIDIT. 1·63. Æ

78. **Nordenskiöld, Adolphus Ericus, Baron.** Bust of Nordenskiöld, *r.*, bare, hair short. *Leg.* ADOLPHUS ERICUS NORDENSKIÖLD . Below, W. RUNEBERG C . JAHN SC.

Rev. The Genius of Science, laureate, standing, *r.*, partially clad

in loose drapery. In her right hand she holds aloft a lamp illuminating the north polar region of a globe beneath. Near figure, an anchor, with compass, a ship's log, and other navigational instruments. *Leg.* ASIA CIRCUM = NA = VIGATA. *Ex.* IN HONOR . POPULARIS SUI SOC . SCIENT . FENNICA CUD . CUR. Below, AHRENBERG DEL. W . RUNEBERG C . JAHN SC.

This medal was struck by the Société des Sciences de Finlande, in honour of Baron Nordenskiöld, and an example in gold was presented to him January 13, 1881. 2·21. Æ.

79. **Parkes, Edmund Alexander, F.R.S.** Head of Parkes, *l.*, bare, *Leg.* EDMUND ALEXANDER PARKES . B . 1819–D . 1876. Below truncation, J. S. & A. B. WYON.

Rev. Within a laurel wreath, PARKES MEMORIAL MEDAL. Inscribed without, 'Η ΠΕΡΙ ΤΟ ΣΩΜΑ ΚΑΙ ΤΗΝ ΨΥΧΗΝ 'ΥΓΙΕΙΑ. 2·2. Æ.

80. **Philadelphia. Numismatic and Antiquarian Society.** Medal struck to commemorate the 21st anniversary of the foundation of the Society. Bust of Eli K. Price, *l.*, hair long, in coat, collar and tie. *Leg.* ELI K. PRICE PRESIDENT. Below, 1879. On truncation, W. H. KEY F.

Rev. The arms, crest, and motto (*vestigia rerum sequi*) of the Society. Inscribed within a border, THE NUMISMATIC & ANTIQUARIAN SOCIETY OF PHILADA . FOUNDED JAN. 1. 1858. 1·66. Æ.

Only one copy was struck in silver, which was presented to the President himself; in bronze, 199 were issued.

81. **Presl, Johann Svatopluk,** and **Karl Bořivoj Presl.** Busts, opposite each other, of K. B. Presl and J. S. Presl. The former in profile, *r.*, hair short, wearing coat, collar, and cravat; the latter, three-quarter face to left, hair parted in middle, wearing coat, collar, and cravat. *Leg.* CAROLVS . BORZVVOJ . PRESL . NATVS . PRAGAE . XVII . FEB . A . MDCCLXXXXIIII . MORTVVS IBIDEM . II . NOV . A . MDCCCLII . DR . MED . ET . PHIL . PROF . P . O . VNIV . PRAGENSIS .

JOANNES . SVATOPLVK . PRESL . NATVS . PRAGAE . IIII . SEPTEMB . A . MDCCLXXXXI . MORTVVS . IBIDEM . VI . APRIL . A . MDCCCXXXXVIIII . DR . MED . PROF . P . O . VNIV . PRAGENSIS. Below the busts, IN . MEMORIAM . JOANNIS . ANTE . HOS . CENTVM . ANNOS . NATI.

Rev. A branching tree fern. Inscribed around, FRATERNIS . ET . NATVRAE . ET . DISCIPLINAE . VINCVLIS . CONIVNCTI. 3·4. Æ.

82. **Pulteney, William, Earl of Bath.** Bust of Pulteney, *r.*, hair long, in loose mantle. *Leg.* GUILIELMUS PULTENEY . A DASSIER F.

Rev. Within a wreath of oak, COMES DE BATH . MDCCXLIV. 2·15. Æ.

83. **Purkyně, Johann E., For. Mem. R.S.** Bust, *r.*, hair short, in coat, collar, and bow tie. *Leg.* JOANN . EV . PURKYNĚ. Below, SEIDAN.

Rev. Inscription PHYSIOLOGIAE RECENTIORIS FUNDATORI DECEM ABHINC LUSTRIS UNIVERSITATI CAROLO - FERDINANDEAE ADLECTO FACULTAS MEDICA PRAGENSIS IX . DEG. MDCCCLXVIII. 1·74. Æ.

84. **Quetelet, Lambert Adolphe Jacques, For. Mem. R.S.** Head of Quetelet, *l.* *Leg.* ADOLPHUS QUETELET. Below, BRAEMT F.
Rev. Inscription, spaced on the face, ADOLPHO QUETELET VIRO DE ACADEMIA EGREGIE MERITO QUINQUE LUSTRA IN ACTUARII PERPETUI MUNERE FELICITER PERACTA CONGRATULANTES HUNC NUMMUM PIETATIS ET REVERENTIAE TESTEM CUDENDUM CURAVERUNT ACADEMIAE REGIAE BELGICAE SOCII ANN. MDCCCLX. 1·75. Æ.

85. **Rotterdam. Bataafsch Genootschap der Prœfondervindelijke Wijsbegeerte.** Medal struck in celebration of the Batavian Society's centenary, 1769–1869. *Inner leg.* Within a circle formed by a coiled snake, SOCIETAS PHILOSOPHIAE EXPERIMENTALIS BATAVA ROTERODAMI CENTESIMUM NATALEM CELEBRANS. *Outer leg.* IN MEMORIAM STEPHANI HOOGENDIJK FUNDATORIS; MDCCLXIX–MDCCCLXIX.
Rev. Experience, in loose draperies, standing, looking to left. In her left hand a crowned staff, with scroll entwined bearing the legend, RERVM MAGISTRA; in right an anchor. Near, a column, on the top of which a pair of scales, on the front the Netherlands arms surmounted by a crown. On the left of the figure an altar, with flames arising, on front the Netherlands lion rampant, on a shield. Inscription, CERTOS FERET EXPERIENTIA FRUCTUS. *Ex.* J. P. MENGER F. 1·59. Æ.

Rumford Medal. *See* London, Royal Society.

86. **Schemnitz. Königl. Ungarische Berg- und Forst-Akademie.** Medal in celebration of the 100th anniversary of the Academy, 1770–1870. Bust of Maria Theresia, Queen of Hungary, *r.*, wearing bandeau, with falling drapery gathered at the breast. On either side a laurel and palm branch. *Leg.* within a border, A . MARIA . THERESIA . HUNG: REGE . METALLICORUM . ACADEMIA. Below, C. RADNITZKY.
Rev. Inscribed within a border, SCHEMNICII . CONDITA . 1770 . PRIMUM . SECULUM . CELEBRAT . 1870. 2·74. Æ.

Siemens, Sir Charles William, F.R.S. *See* London, City and Guilds Institute, *and* King's College.

87. **Sloane, Sir Hans, Bart., P.R.S.** Bust of Sir Hans Sloane, *l.*, cap on head, in loose robe. *Leg.* HANS SLOANE EQU . BARONETTUS. Below, A. DASSIER . F.
Rev. Inscription, PRÆSES SOCIETATIS REGIÆ LONDINENSIS . MDCCXLIV. Above, festoons of flowers; below, branches of oak. 2·15. Æ.

88. ——— ——— ——— ——— Another copy. 2·15. Æ.

89. **Soane, Sir John, F.R.S.** Bust of Soane, *r.*, bare, hair short. *Leg.* JOHN SOANE. Below, W . WYON . A . R . A . MINT.
Rev. A representation of the elevation of the north-west angle

of the Bank of England, with inscription, A TRIBUTE OF RESPECT FROM THE BRITISH ARCHITECTS. Below, in exergue, MDCCCXXXIV. 2·26. Æ.

Solly, Samuel, F.R.S. *See* London, St. Thomas's Hospital.

90. **Stas, Jean Servais, For. Mem. R.S.** Head of Stas, *l.*, Below, A . MICHAUX . D'APRES L.W.
Rev. Inscription, spaced on the field, A JEAN-SERVAIS STAS, NÉ A LOUVAIN LE 21 AOUT 1813, ÉLU MEMBRE DE LA CLASSE DES SCIENCES EN 1841 . SOUVENIR JUBILAIRE (5 MAI 1891). Inscribed around, within a border, ACADÉMIE ROYALE DES SCIENCES, DES LETTRES ET DES BEAUX-ARTS DE BELGIQUE. 2·02. Æ.

91. **Stukeley, William, F.R.S.** Head of Stukeley, *r.*, with wreath of oak leaves. *Leg.* REV . GVL . STVKELEY . M.D . SR & AS. Below truncation, *æt.* 54.
Rev. Representation of Stonehenge, and below, OB . MAR . 4 . 1765. Æ : 84. Cast. 3·32. Æ.

92. **Sydney. Royal Society of New South Wales.** Bust of Rev. William Branwhite Clarke, F.R.S., *r.*, bearded, wearing academicals. *Leg.* WILLIAM BRANWHITE CLARKE . M.A. F.R.S. 1878. Below. J. S. & A. B. WYON.
Rev. Within a wreath, composed of the palms and flowering plants of Australia, FOR RESEARCHES IN NATURAL SCIENCE. Inscribed around, THE ROYAL SOCIETY OF NEW SOUTH WALES . SYDNEY. Below, J. S. & A. B. WYON. 2·18. Æ.

93. **Sylvester, James Joseph, F.R.S.** Bust, *l.*, hair and beard long, crown of head bare, in coat and collar. Behind, SYLVESTER. Below truncation, C. E. BARBER F.
Rev. Within a wreath of oak leaves, INDE . AB . A.D . MDCCCLXXVI . VSQVE . AD . A.D . MDCCCLXXXIII. Inscribed around within a border, PER . SEPTEM . ANNOS . IN . VNIVERSITATE . AB . IOHNS . HOPKINS . FVNDATA . PROFESSOR. 2·52. Æ.

94. **Thiersch, Frederick von.** Bust of Thiersch, *r.*, hair short, bare. Below, J. RIES.
Rev. Within an ornamental border, FRIDERICVS THIERSCH PHILOLOGVS. Inscribed without, NATVS D . XIV . M . JVNII MDCCLXXXIV . OBIIT D. XXV. M . FEBRVARII MDCCCLX. 1·89. Æ.

95. **Tiedemann, Friedrich, For. Mem. R.S.** Bust of Tiedemann, *r.*, bare. *Leg.* FRIDERICVS TIEDEMANN NAT . D. XXIII AVG . MDCCLXXXI. Below, C. VOIGT.
Rev. A star-fish, with inscription VIRO DE AVGENDA NATVRAE SCIENTIA PER X LVSTRA EGREGIE MERITO SODALES. FRANCOF . A . M. D . X MART . MDCCCLIV. 1·76. Æ.

Todd, Robert Bentley, F.R.S. *See* London, King's College.

96. **Upsala. Universitet.** Medal struck in celebration of the 400th anniversary of the University. Head of Oscar II of Sweden and Norway, *r.*, bare, hair short. *Leg.* OSCAR II REX SVECIÆ ET NORVEGIÆ. Below, A. LINDBERG.

Rev. The Genius of Upsala, laureate, clad in loose robes, is seated, facing. In her right hand she holds aloft the lamp of knowledge, the left rests upon the triangular crowned shield of Svealand, the head of a crouching lion appearing from behind (in allusion to Götaland). On the left of the figure emblems of the arts; above the pole star casts its rays. In the field a flying bat. Inscription, EX TENEBRIS PER UMBRAS AD LUCEM. *Ex.* PERACTA QUATUOR SECULA CELEBRAVIT UNIVERSITAS UPSALIENSIS MDCCCLXXVII. LINDBERG. 2·22. Æ.

97. **Wales, Frederick, Prince of, F.R.S.** Bust of the Prince, *l.*, hair long, in armour, riband and star of the Garter. *Leg.* FREDERIC. WALLIÆ PRINCEPS. Below, J. A. DASSIER.

Rev. Two genii, among clouds, supporting the Prince's coronet, with plumes and motto. 2·15. Æ.

98. **Watt, James, F.R.S.** Bust of Watt, *r.*, with mantle over the shoulders. Behind, in the field, JAMES WATT 1736–1819. Below, JOSEPH S. WYON S.

Rev. Representation of a steam engine, with sun and planet motion, and inscription below, STEAM ENGINE AS CONSTRUCTED BY JAMES WATT. 1·86. Æ.

99. —— —— —— Head of Watt, *l.*, behind head, I WATT. On truncation, A. J. STOTHARD.; below, F. L. CHANTREY. R.A. D.

Rev. Clio, looking to *r.*, leans in almost upright position against low pillar, on which rests her left hand, with scroll bearing legend, TO GREAT MEN; in right hand a pen. Below, PUB^D BY S PARKER LONDON. MDCCCXXVII. Right and left, A. J. STOTHARD F. T. STOTHARD R.A. D. 2·46. Æ.

—— —— —— *See* also Montreal, McGill University.

100. **Whitworth, Sir Joseph, Bart, F.R.S.** Bust of Whitworth, *l.*, bearded, hair long, crown of head bare, in collar and coat. *Leg.* SIR JOSEPH WHITWORTH . BART .·. F R S .·. D C L .·. L L D B^N ⁘ DEC^R. XXI .·. MDCCCIII. On truncation, 18EJP83. Below, ALLAN WYON.

Rev. A representation of Whitworth's measuring machine. Inscription above, A DIFFERENCE OF ONE MILLIONTH OF AN INCH IS MEASVRED BY VSING FOVR TRVE PLANES IN CONCERT; beneath, WHITWORTH SCHOLARSHIPS FOVNDED MDCCCLXVIII; to right, J. S. & A. B. WYON. 2·26. Æ.

101. **Wray, Daniel, F.R.S.** Bust of Wray, *r.*, hair short, in mantle fastened with brooch on the shoulder. *Leg.* DANIEL . WRAY . ANGLVS . AET . XXIV. On truncation, 1726. Below, G . POZZO . F.

Rev. Inscription, NIL ACTVM REPVTANS CVM QVID SVPERESSET AGENDVM. 2·7. Æ.

102. **Wren, Sir Christopher, P.R.S.** Bust of Wren, *l.*, hair long, in vest and loose mantle. *Leg.* . CHRISTOP . WREN . EQVES . AVR & ARCHITECT . Below bust, . OBIIT . A . D . 1723. ÆT. 91 .

Rev. The west front of St. Paul's Cathedral. *Upper leg.* VNVM . PRO . CVNCTIS . FAMA . LOQVATVR . OPVS . *Lower leg.* INCEPT . A.D . 1675 . PERFECT . A.D . 1711. *Ex.* AEDES . S . PAVLI . LOND. ; G . D . GAAB . SCVLP. Cast. 3·92. Æ.

OBITUARY NOTICES OF FELLOWS DECEASED.

SIR JOHN HAWKSHAW, civil engineer, was born at Leeds in 1811, and was educated at the Grammar School of that town. After serving a pupilage to an engineer of large practice in Yorkshire, he went to South America for a few years to superintend some large copper mines, and soon after his return succeeded George Stephenson as engineer to a railway between Manchester and Leeds. This led to a further connexion with the railways which afterwards expanded into the great group called the "Lancashire and Yorkshire" system, and he soon took a position as one of the most eminent railway engineers in the country.

After he established himself in London, his practice extended to other branches of engineering, and about 1856 he succeeded Mr. J. M. Rendel in directing the construction of the great Harbour of Refuge at Holyhead. He made, with the sanction of the Government, extensive alterations of the original design, and, in consideration of his important services on the work, he received in 1873 the honour of knighthood.

Among other large works of his in Great Britain may be mentioned docks in London, Hull, and Fleetwood; a main drainage system for Brighton; waterworks for Dublin; important improvements in the drainage of the Fen districts; the foundations of the great iron forts at Spithead, and the tunnel, $4\frac{1}{3}$ miles long, lately formed under the Severn. He also devoted much attention to the proposed great tunnel under the Straits of Dover, and considered he had favourably solved the question from an engineering point of view. But, subsequently, he doubted the expediency, on grounds of national policy, of forming such a connexion between the two countries, and withdrew his support from the scheme.

He is best known to Londoners by his extension of the South Eastern Railway from London Bridge to new termini at Charing Cross and Cannon Street, a very difficult and expensive work, cutting through the heart of London, and requiring two large new bridges across the Thames. And though artistic critics doubt whether this has contributed, like the Thames Embankment, to the embellishment of the Metropolis, there can be no question that it has been of immense benefit to the inhabitants; and Sir John always held that æsthetic considerations were out of place if they interfered with works of public utility.

He was also largely engaged on foreign works. Towards the end of 1863 he visited Egypt, at the request of the Viceroy, to report on doubtful points respecting the Suez Canal, and his emphatic recommendations led largely to its completion. He executed a great ship canal in Holland, made designs for navigating the First Cataract of the Nile, and had to do with railways and other large works in Russia, India, the Mauritius, and Brazil.

He was one of the most active members of the Institution of Civil Engineers, and occupied the position of President in the years 1862 and 1863.

Sir John was not merely a railway maker; he paid much attention to general principles, and some that he strongly advocated may be mentioned. One was the allowance of greater latitude in regard to gradients. The earlier engineers thought that almost any cost should be incurred for the purpose of getting a road as flat as possible; and for very heavy traffic to be carried very cheaply this is always true. But Sir John urged that in a vast number of cases it was preferable to adopt steeper gradients, and so to save original outlay. He relied on the mechanical skill of engineers to work such gradients effectively and safely, and contended that this principle would lead to a great extension of the system in districts where it would be otherwise impracticable. It is remarkable how his predictions have been verified. In the days of George Stephenson, 1 in 264 was thought very steep, and 1 in 100 was said to require stationary engines. Now 1 in 100 is thought nothing of, and 1 in 40 or 50 is allowed for important lines, while for mountain districts we find gradients of 1 in 4, or even steeper still.

Another principle he advocated was that, when the traffic became very large, railways could never be worked to their full advantage unless special lines were allotted to special speeds, thus separating the quick from the heavy traffic. It was a long time before practical railway managers appreciated this idea, but the pressure of increased traffic has lately forced it on them, and it is now being extensively carried out by duplications of many great lines.

In 1875–76 Sir John filled the honourable office (succeeding Professor Tyndall) of President of the British Association, and gave his opening address at Bristol on the 25th August, 1875. He began by saying:—

"Past Presidents have already discoursed on many subjects—on things organic and inorganic—on the mind, and on things, perhaps, beyond the reach of mind; and I have arrived at the conclusion that humbler themes will not be out of place on this occasion.

"I propose in this address to say something of a profession to which my lifetime has been devoted—a theme which cannot, perhaps, be expected to stand so high in your estimation as in my own, and I

may have some difficulty in making it interesting; but I have chosen it because it is a subject I ought to understand better than any other."

Half the address was devoted to the history of works of a nature corresponding to those of modern civil engineering, in a long series, comprising those of the Egyptians, the Assyrians, the Peruvians, the Hindoos and Mahomedans, the Chinese, the Carthaginians, the Greeks, and the Romans, down to the present day. Modern engineering works were then alluded to, particularly steam navigation and the electric telegraph; and in regard to the speaker's own special subject, railways, he dwelt more on economical than constructive views. He pointed out in a striking manner the great benefits that they had conferred on mankind. He said:—

"Railways add enormously to the national wealth. It may be safely assumed that the railways in the British Islands now save the nation a much larger sum annually than the gross amount of all the dividends payable to the proprietors, without at all taking into account the benefit arising from the saving in time. The benefits under this head defy calculation, and cannot, with any accuracy, be put into money. But it would not be at all over-estimating this question to say that in time and money the nation gains at least what is equivalent to 10 per cent. on all the capital expended."

He argued from this that even where a railway would only yield a small dividend to its proprietors, it was to the national interest that it should be carried out by Government aid.

He also alluded to the subject of safety in railway travelling:—

"It is well that the elements on which this depends should be clearly understood. It will be thought that longer experience in the management of railways should go to ensure greater safety, but there are other elements of the question. It depends on the perfection of the machine in all its parts; it depends also on the nature and quantity of traffic; and, lastly, on human care and attention; for so many of these accidents as arise from the fallibility of men will never be eliminated until the race be improved."

He, however, gave some remarkable statistics to show how minute the risk of accident really is, and quoted the saying of a former President of the Board of Trade that he felt safer in a railway carriage than anywhere else. "And," added Sir John, "he was not far wrong."

He took interest in geology, and published, in 1842, some good descriptions of fossil footsteps and fossil trees that had been discovered in works under his care. He further presented to the Manchester Geological Society, in 1843, a somewhat elaborate theoretical paper on the Origin of the Deposits of Coal.

He was elected into the Royal Society, by the propositions of

many eminent Fellows, on the 7th of June, 1855. He served three times on the Council, namely, in 1868–69, in 1874–75, and in 1881–82.

In his later years he gave up active practice; but he retained his faculties to the last, and he died, at the ripe age of 80, on the 2nd of June, 1891.

W. P.

Peter Martin Duncan was born at Twickenham in 1824, and received his early education in the Grammar School of that village, once the home of Walpole and of Pope. He was afterwards placed for a short time in a school in Switzerland. On his return to England, he entered the Medical Department of King's College, London, in 1842. Here he received his formal scientific training, taking his degree of M.B. London in 1846, and in 1849 he was elected an Associate of his College. After acting for a time as assistant to a doctor at Rochester, he removed to Colchester, where a practice had been purchased for him. Here he resided for many years, and published his first scientific essay, which consisted of "Observations on the Pollen-tube, its Growth, Histology, and Physiology" (1856). But he did not at Colchester secure much time for original research, for most of that which was left him by his profession was occupied by work in connexion with the municipality. During his residence he filled the office of Mayor, thus proving that he had won the confidence of his fellow-townsmen, while the admirable arrangement of the local Museum, which under his direction was reorganised upon lines far in advance of the time, is a sign of his interest in the educational institutions of the town. About 1860 he took a practice at Blackheath, when he was able to spare more time for scientific work, devoting himself to the study of fossil Corals; and, as his interest deepened in the problems which they presented to him, he was led to abandon the lucrative prospects offered by his profession, and to devote himself entirely to original research. In this he was no doubt encouraged by the reception accorded to his first palæontological papers, which were read in 1863, and gained for him recognition as a most able palæontologist. In the following year he was appointed one of the honorary secretaries of the Geological Society, and two years later, he was elected a Fellow of the Royal Society.

After leaving Blackheath, he settled near Regent's Park; but he was not long allowed to remain in retirement, for in 1870 he was called to the Chair of Geology at King's College, and a Fellowship followed in 1871. Shortly afterwards he accepted also the Professorship of Geology at Cooper's Hill, both of which appointments he held till his death. He resigned the Secretaryship of the Geological Society in 1870, after a seven years' tenure of office, and in 1872 he was elected a Vice-President, and President in 1876 and

1877. In 1881 he was awarded the Wollaston Medal, the highest honour which the Geological Society can bestow. Though he was most closely connected with the Geological Society, he was an influential member of other scientific bodies; he served on the Council of the Royal Society from 1876 to 1878, was President of the Geological Section of the British Association in 1879, and of the Microscopical Society from 1881 to 1883.

On turning to Professor Duncan's scientific work, one is impressed by the enormous amount he accomplished, and the wide range of his interests and influence. His first paper (1856) was botanical, and he long retained his attachment to this subject, his last paper on vegetable physiology being published in 1874; while, still later on, he worked out the parasitic Algæ which he discovered in some Silurian Corals. His first important work was the series of five memoirs on the Fossil Corals of the West Indies. The subject was full of difficulties; the living Corals of the area were but little known, so that the materials for the comparison of the recent and fossil faunas were quite insufficient. But Professor Duncan attacked the subject with characteristic energy, and his sound common sense enabled him to avoid many a pitfall; his memoir was certainly a most valuable addition to our knowledge of the later Tertiary Corals. This work was followed by a long list of memoirs, in which he describes the Coral faunas (especially the Cainozoic) of England, Australia, Tasmania, India, Java, Arabia, and Malta. His "British Fossil Corals" is probably one of the best contributions published by the Palæontographical Society; being so much more modern in its method, and more thorough in its treatment, than the work to which it was issued as a supplement.

But though Professor Duncan's interests were probably at first rather zoological than geological, he soon became absorbed in the line of work which he had been led by circumstances to select. He soon realised that the description of the anatomical structure and the determination of the systematic position of a fossil did not constitute the sole duties of a palæontologist; with him these were but preliminary to the consideration of the affinities of faunas and their bearing on the physical geography of the past. He was a palæontologist in the truest sense of the word—not a morphologist who happened to study extinct forms, but a geologist who used fossils as a petrologist uses minerals. Hence his early work on the West India Corals commenced by a detailed study of their conditions of fossilisation, and closed by a discussion of their evidence as to the Cainozoic physiography of the Caribbean region; similarly, his later studies of the European Corals led to his striking paper on "The Physical Geography of Western Europe during the Mesozoic and Cainozoic Periods elucidated by their Coral Faunas."

In his later study of the Echinoidea, he commenced with those in beds the Corals of which he had already examined, among the most remarkable being those from South Australia, which he described in a series of papers dating from 1864 to 1887. It was apparently his interest in the origin of this fauna, with its mixture of Cretaceous and Cainozoic genera, that led him to take up the Indian Echinoids, which, in conjunction with Mr. W. Percy Sladen, F.L.S., he monographed with such detail and care.

He studied with especial interest the Echinoids of the Cenomanian, and by the aid of the small collections of the Rev. W. F. Holland, in Sinai, and of Dr. Carter, in South Arabia, he gradually built up the connexion between the European fauna and that of Northern India. By his comparison of those of the Peninsular and Extra-Peninsular areas he demonstrated the existence of the land-barrier that stretched across India, and away to the south-west, of which such important use has been made in recent controversy. His views on geographical distribution were original, and had been carefully matured; his lecture on "The Formation of the Main Land Masses" showed that he did not accept the views of the permanence of oceans and continents, a subject upon which he was competent to speak with authority. His paper on "The Fauna of the Alpine Lakes" probably dealt the most serious blow ever struck to the theory of the glacial origin of the Swiss lake-basins.

But though Professor Duncan did not regard morphology as the highest end, he did not by any means neglect it; thus our knowledge of the perignathic girdle of the Echinoids and its value in classification we owe mainly to him; while his remarkably suggestive and original essay on the structure of the ambulacra of the regular Echinoidea, perhaps his most masterly piece of work, has gained the highest praise even from men opposed to his views.

In addition to his contributions to palæontology, he has done much in zoology; he wrote a series of papers on the anatomy of the Temnopleuridæ, Saleniidæ, and other groups of the Echinoidea, and described, amongst others, the Madreporaria of the "Porcupine" Expedition, the Ophiurids and Corals of Mergui, and, in conjunction with his constant collaborator, Mr. Sladen, the Echinodermata from Greenland. Two of his most valuable works are "The Revision of the Madreporaria," and his "Revision of the Genera and Great Groups of the Echinoidea." The former was issued in 1885, and consisted of diagnoses of every genus of Coral (excluding the Rugosa), and of a classification which has not yet been supplanted. His revision of the Echinoidea made a great advance in our knowledge of every order. The application of his own discoveries on the ambulacral structure enabled him to bring the Palechinoidea from chaos into order, and to replace the artificial arrangement of the Diadema-

tidæ by a natural classification; his previous detection of the fundamental differences between the pits of *Temnopleurus* and the fossettes of *Temnechinus* gave him the clue to the arrangement of that group; and his substitution of positive for comparative diagnoses in many recent genera has greatly aided the comparison of the fossil and deep-sea types. By these two revisions alone Professor Duncan has earned the gratitude of every palæontologist and zoologist, and has given a firm basis for future work. They are indispensable works of reference to every student of these groups.

In addition to the Corals and Echinodermata, Professor Duncan made some contributions to the study of the Protozoa and Sponges, while his clearness as a teacher led him to undertake a good deal of lecturing and popular literary work; thus he edited the six volumes of 'Cassell's Natural History,' and, amongst others, wrote a primer of physical geography, a volume of biographies of the 'Heroes of Science,' a paper on Voltaire's attitude to geology, and edited recent issues of Lyell's 'Student's Elements.'

To his first love, the Corals, he proposed to return on the conclusion of his revision of the Echinoidea; he commenced work upon a large Indian collection, and planned a supplement to his revision of the Madreporaria, in which he intended to discuss recent criticisms and incorporate subsequent progress. But it was not to be. He was smitten with disease, and, after a long and painful illness, quietly passed away on the early morning of the 28th of May.

The fine, keen sense of humour, which remained unblunted almost to the last, the genial kindness with which he was ever ready with help, especially to younger men, united with the recognition of his sterling worth and sound judgment, gained him wide popularity and esteem.

Henry Martyn Jeffery was the only son of Mr. John Jeffery, of Gwennap, Cornwall, a parish situated about midway between the towns of Redruth and Penryn. He was born on January 5, 1826, at Lamorran, near Truro, on the banks of the River Fal, at the rectory of his maternal grandfather, the Rev. W. Curgenven, who married the sister of the distinguished Orientalist and missionary, the Rev. Henry Martyn, B.D., the Senior Wrangler in 1801. Mr. Jeffery was also related to the family of the Rev. Malachy Hitchins, Vicar of St. Hilary, near Marazion, the comparer of the "Nautical Almanac," under Dr. Maskelyne, from 1767 to 1809, and one of the observers of the transit of Venus at the Royal Observatory in 1769. Mr. Jeffery always referred with a natural pride to these two well-known mathematical members of his family.

The early years of Mr. Jeffery were mostly spent at his father's home at Gwennap, but from the age of seven to fourteen he was a pupil at

the Falmouth Grammar School. On leaving this school, in 1840, he exhibited undoubted signs of considerable mathematical and classical ability—so much so that he offered himself as a tutor in a private gentleman's family. The writer of this notice has seen a copy of his letter containing a list of the subjects which he considered himself competent to teach, and from it we may gather that he was really an intelligent youth with more than usual precocity. Fortunately for himself, he was, at the advice of some friends, sent in 1841 to the Grammar School at Sedbergh, Yorkshire, where he was trained by the Rev. J. H. Evans, a late Fellow of St. John's College, Cambridge. Here he remained until 1845. In October of that year he entered as an undergraduate at St. John's College, but soon after migrated to St. Catharine's College, graduating as B.A. in 1849 in the Mathematical Tripos as Sixth Wrangler, and in the Classical Tripos in the Second Class. He proceeded to the degree of M.A. in 1852, and in that year he was adjudged the special distinction of bracketed first Tyrwhitt Hebrew Scholar.

Soon after taking his degree, Mr. Jeffery accepted the post of Lecturer in the College of Civil Engineers at Putney, and in 1852 he was selected by the President and Fellows of Corpus Christi College, Oxford, to fill the office of Second Master of Pate's Grammar School, at Cheltenham. Sixteen years after, on the resignation of the Rev. Dr. Hayman, in 1868, he was appointed to succeed to the vacant Headmastership, an office which he retained with success until his retirement in 1882. Many of his pupils have acknowledged their indebtedness to Mr. Jeffery for their general success in life, some of whom have attained high distinction at the Universities, and in various competitive examinations for admission into the public service.

Although, while at the Cheltenham Grammar School, Mr. Jeffery's official time was more especially devoted to the classical department, it is as a pure mathematician that his name will be most remembered. Shortly after he permanently settled in Cheltenham he commenced the long and continuous series of investigations in pure mathematics which have enriched the pages of the 'Quarterly Journal of Pure and Applied Mathematics,' the 'Proceedings of the London Mathematical Society,' the 'Reports of the British Association,' and other scientific journals. His most important papers have been on pure analysis and analytical geometry, especially on the classification of class-cubics, both in plane and spherical geometry. Instalments of the similar classification for class-quartics have also been published. He had been for some time engaged on the continuation of this work. The titles of a few of his numerous papers will give a sufficient indication of the general character of his investigations:—"Two Theorems in Permutations and Combinations, and a Theorem in Con-

gruencies"; "The Spherical Ellipse referred to Trilinear Coordinates"; "Cubics of the Third Class with Triple Foci, both Plane and Spherical"; "Spherical Class Cubics with Double Foci and Double Cyclic Arcs"; "On Sphero-Cyclides"; "On the Identity of the Nodes of a Nodal Curve of the Fourth Order with those of its Quartic and Sextic Contravariants"; and "On the Genesis of Binodal Quartic Curves from Conics." It appears to have been Mr. Jeffery's intention to prepare a text-book on his favourite subjects. Some progress was made in the preparation of such a work, and he was looking forward with considerable interest to the publication of a treatise which he hoped would prove useful to the student of the higher mathematics. Only last summer, while the writer was enjoying his hospitality, Mr. Jeffery exhibited to him a huge quantity of mathematical manuscript, beautifully written out for the press, in the preparation of which all his recent leisure hours had been devoted. He was anticipating with evident enthusiasm the prospect of an early completion of his labours in this branch of pure mathematics by the production of a text-book; but, alas! his wishes can never be realised, for the small portion of the work prepared for the press exists only as a fragmentary record of his mathematical talents, and of the studious activity of his life to the end. His last original paper was communicated to the London Mathematical Society only a few weeks before his fatal illness, and it was read at the meeting of the Society on November 12, nine days after his decease. In addition to his mathematical work, Mr. Jeffery has occasionally been occupied in other fields of labour, mostly in classics, archæology, and topographical history. In 1853 he wrote, as a coadjutor with Dr. E. R. Humphreys, on classical composition in Greek iambics and Latin prose.

On his retirement from Cheltenham Grammar School Mr. Jeffery, who was never married, took up his residence at Falmouth, partly that he might be in a convenient locality to undertake the management of a considerable amount of house property inherited from his father, and partly on account of the comparatively mild winter climate of his native county. Here he identified himself with the active management of several local scientific institutions, especially of the Royal Institution of Cornwall, at Truro, and the Royal Cornwall Polytechnic Society, at Falmouth, in both of which he had filled the office of Vice-President, and was a valued contributor to their journals. His paper on the "Early Topography of Falmouth," in the 'Journal of the Royal Institution of Cornwall,' is a most important contribution to the local history of that part of Cornwall. Mr. Jeffery was the Honorary Secretary of the new Falmouth Observatory, in which he has taken a great interest since its foundation. Mr. Kitto, the Superintendent, has remarked that he was much

indebted to him for assistance in the initial difficulties of the magnetograph work, a department of the Observatory to which Mr. Jeffery paid a constant personal attention. He also retained much affection for the Falmouth Grammar School, where he received his early education, and this he was always ready to show by his advice and pecuniary support. His loss will be severely felt by all these institutions.

Mr. Jeffery was elected a Fellow of the Royal Society on June 3, 1880, but, owing to the distance of his residence from London, he rarely had an opportunity of attending the meetings. It was, however, a great delight to him to spend a few weeks in London each year, and he usually chose the months of May or June, so that he might enjoy the pleasing association with his scientific friends at one of the annual *conversaziones*. He also took great interest in the meetings of the British Association for the Advancemement of Science, at which he was a frequent attendant, and a contributor of papers. For some years past Mr. Jeffery was troubled, more or less, with an internal complaint which occasionally caused him considerable personal inconvenience, and, latterly, he suffered from the effects of insomnia, but still he remained active to within a fortnight of his death, often walking from Falmouth to Truro, a distance of about nine or ten miles, without any apparent fatigue. He was a great lover of long-walking exercise, and, even within a few weeks of his death, though in ill-health, he took a wearying walk of about twelve miles. When the writer visited him in the past summer, Mr. Jeffery appeared to be in better health than usual; but in the middle of October the disease became much aggravated, necessitating an operation, and, after a short illness, accompanied by much suffering, he gradually sank. On the Saturday before his death he became partially unconscious, and on the Monday following wholly so, and in this condition he passed away, peacefully, on the morning of Tuesday, November 3, 1891, in the sixty-sixth year of his age. Three days afterwards his remains were interred in the family vault, with his father and mother, at Gwennap, the country home of his early youth.

E. D.

Henry Bowman Brady, LL.D., was born in 1835. He was the second son of Henry Brady, of Gateshead, who for fifty years carried on an extensive practice as surgeon in that town. He was educated at the schools of the Society of Friends, at Ackworth, and at Tulketh Hall, near Preston. His father was a naturalist, and instilled into his son a love of nature, which was fostered at his first school; but the influence that shaped his mature career came from the colony of naturalists which has had its headquarters at Newcastle-on-Tyne for several generations. The names of Bewick, Alder, Albany and John

Hancock, and others are those of men whom Newcastle has contributed to the roll of English naturalists, and the Brady family would seem to have been thoroughly permeated with the local enthusiasm for the study of natural history.

On leaving school, in 1850, Brady was apprenticed to the late Mr. Thomas Harvey, pharmaceutical chemist, of Leeds, and in 1855 he entered upon business for himself in Newcastle-on-Tyne.

His conspicuous ability soon gained for him the support of the medical profession and the public, and he laid the foundation of the very extensive business in wholesale and retail pharmacy and scientific apparatus subsequently conducted by the firm of Brady and Martin. During the twenty-one years of his business life, Mr. Brady was closely identified with the Pharmaceutical Society, and he became the President of the British Pharmaceutical Conference in 1872. He was for many years on the Council of the Pharmaceutical Society, and greatly contributed to the progress of that body by developing the scientific education of pharmaceutical chemists.

His more direct contributions to science were in the form of researches in natural history, especially on the Foraminifera. His first publication seems to have been a contribution, in 1863, to the British Association, as a report on the dredging of the Northumberland coast and Dogger Bank; his last was a paper which appeared only a short time ago, on the minute organisms with which his name will always be connected. Between these two he published a large number of researches, including a monograph on Carboniferous and Permian Foraminifera, an exhaustive report on the Foraminifera of the "Challenger" Expedition, as well as monographs on *Parkeria* and *Loftusia*, and on *Polymorphina*, in which he was joint author with Mr. W. K. Parker, F.R.S., and Professor T. Rupert Jones, F.R.S. The report on the Foraminifera is embodied in two quarto volumes, one containing 814 pages of text, and the other 114 plates, which possess great artistic merit. The bibliography of the subject alone occupies forty-six pages of the first volume. The illustrations of such works are of much importance, and the author gave to this department of his work the fastidious care of a skilled draughtsman. By these works he not only established a position both in this country and abroad as one of the highest authorities on the subject, but, what is of more importance, largely advanced our knowledge. Every one of his papers is characterised by the most conscientious accuracy and justice; and though his attention was largely directed to classification and to the morphological points therein involved, his mind, as several of his papers indicate, was also occupied with the wider problems of morphological and biological interest which the study of these lowly forms suggests.

In 1874 he was elected a Fellow of this Society, and in 1888 served

on our Council. In the same year the University of Aberdeen conferred upon him the degree of LL.D., in recognition of his scientific work, and he also received from the Emperor of Austria a valuable gold medal, as a mark of his appreciation of the valuable assistance which Mr. Brady had rendered to the Hof-Museum.

He was a man of slight physique and delicate health, and in later years he was compelled to leave his business and seek refuge in warmer climates than our own. In his travels he visited the United States of America, the Upper Nile, India, Ceylon, Japan, Java, Australia, New Zealand, and various islands of the Pacific Ocean. His last journey was in the winter of 1890, when, with some friends, he visited Cairo and ascended the Nile. He was laid up at Cairo with œdema of the feet and legs, from which he never quite recovered, but the actual cause of his death, which occurred on the 10th of January, 1891, was a rapid attack of pneumonia.

He accomplished an immense amount of work, which remains as a monument to his unwearied patience and industry. His amiability won for him a large circle of friends, and he could have wished no higher tribute to his memory than that offered by Dr. Michael Foster, who wrote as follows in 'Nature,' January 29th, 1891:—"Science has lost a steady and fruitful worker, and many men of science have lost a friend and a helpmate whose place they feel no one else can fill. His wide knowledge of many branches of scientific inquiry and his large acquaintance with scientific men made the hours spent with him always profitable; his sympathy with art and literature, and that special knowledge of men and things which belongs only to the travelled man, made him welcome also where science was unknown, while the brave patience with which he bore the many troubles of enfeebled health, his unselfish thoughtfulness for interests other than his own, and a sense of humour which, when needed, led him to desert his usual staid demeanour for the merriment of the moment, endeared him to all his friends."

The catalogues of the Royal Society show that, down to 1883, Mr. Brady was the author of thirty papers and monographs. He has bequeathed to the Society the very valuable portion of his library which relates to the study of the Protozoa. This collection, which now forms a distinct section of the Society's Library, and for the maintenance and increase of which he made provision, consists of some 150 volumes, including, besides many older works on the subject of great rarity and value, his extensive series of collected excerpt 'Memoirs and Papers relating to the Foraminifera,' gathered, arranged, and annotated by him during many years of labour.

W. C. R.-A.

Sir George Edward Paget was born at Yarmouth in 1809. He was the seventh of seventeen children, of whom Sir James Paget, Bart., F.R.S., is the only survivor. His early education was at the Charterhouse. He was admitted at Caius College, Cambridge, in 1827, and graduated in Arts as Eighth Wrangler in 1831. He was elected Fellow of his college in 1832, graduated as M.B. in 1833 and as M.D. in 1839, was elected physician to Addenbrooke's Hospital in 1841, and held the office for forty-three years, retiring in 1884, when a marble bust of him was placed in the hospital, as a memorial of his long and valued services. He represented the University of Cambridge on the General Medical Council from 1864 to 1869, and was then chosen President of the Council, from which post he retired in 1874. In 1872 he was appointed Regius Professor of Physic by the Crown, and held the office till his death. He became Fellow of the Royal Society in 1873, and was made K.C.B. in 1885. He became Fellow of the Royal College of Physicians 1839, was made Hon. M.D. Dublin 1867, Hon. D.C.L. Durham 1870, Hon. LL.D. Edinburgh 1871, Hon. D.C.L. Oxford 1872, and was President of the Meeting of the British Medical Association at Cambridge in 1864. His writings were "A Notice of an Unpublished Manuscript of Harvey" in 1850; the "Address as President of the Medical Association" in 1864; the "Harveian Oration" in 1866; and various papers in the medical journals. He married in 1851, and left several children. He died in January, 1892. He was an excellent physician, and enjoyed large practice in and around Cambridge for many years.

He was a man of great ability and firm character, remarkably quick, yet scrupulously accurate, truthful and very cautious, attentive to detail, wise in judgment, and earnest in purpose. By his wisdom, watchfulness, and zeal he largely promoted the success of the Cambridge Medical School; and by his love for his University and his rectitude of character he won the confidence of the men of Cambridge, who all regarded him with respect and affection, and rejoiced in the honour done him by the Queen and by various universities. These qualities and his genial, kind manner gave him a large circle of warm friends. Added to all this, his brightness and cheerfulness, his great stores of accurate information and his inexhaustible fund of anecdotes and stories, the relation of which in his precise and humorous style was most telling, and his fondness of social life made him a delightful companion. He was a spare, brisk, active man, enjoyed good health, and continued conscientiously the duties of his professorship till, having entered his eighty-third year, he succumbed to the influenza in January last.

G. M. H.

Sir James Caird was the son of James Caird, of Stranraer. He was born in 1816, educated at Edinburgh High School and University,

and at an early period turned his attention to those agricultural and economic questions to which he eventually devoted the greater part of his life.

In 1849 appeared the first edition of his work on 'High Farming,' and in the autumn of that year he visited Ireland, which was still suffering from the effects of the famine of 1846–47, and reported to the Government upon the agricultural outlook in that island. In the following year 'The Times' obtained the services of Mr. Caird as commissioner to investigate the condition of agriculture in England. His letters to that newspaper constituted the first general account of English agriculture since the time of Arthur Young, and they afterwards appeared in book form. In 1859 he published an account of a visit to the Prairie Lands of the Mississippi Basin, directing attention to their extraordinary agricultural capabilities.

In 1857 Mr. Caird entered Parliament, and in the session of 1864 he at length carried a resolution in favour of the collection of agricultural statistics. As a result of this vote the Agricultural Returns for Great Britain were commenced. These have been issued annually since 1866, and have proved of the highest value. In 1869 he again visited Ireland and published a pamphlet on the Land Question.

After the great Indian famine of 1876–77, Mr. Caird served upon the Commission which was appointed to enquire into the whole subject, and he afterwards embodied his own views and conclusions in his work, 'India; the Land and the People.'

In 1882 Mr. Caird was knighted, being created K.C.B. In 1886 he joined Earl Cowper's Irish Commission, and in 1889, upon the formation of the new Board of Agriculture, Sir James Caird became a member of the Board, and was appointed a Privy Councillor. One of his last undertakings was the preparation, at the request of the Royal Agricultural Society of England, of an account of the work of the Society during the first fifty years of its existence. This valuable retrospect appeared in 1890, in the opening number of the Third Series of the Society's Journal, under the title of "Fifty Years' Progress of British Agriculture."

Sir James Caird was elected a Fellow of the Royal Society in 1875. He was a J.P. for Kirkcudbrightshire, and a D.L. and J.P. for Wigtonshire. He died in London, February 9, 1892.

Colonel James Augustus Grant, C.B., C.S.I., died at Nairn on the 11th February. He was born at Nairn in 1827, "a son of the manse," being the son of the parish minister. After being educated at the Grammar School, and at Marischal College, Aberdeen, he obtained in 1846 a commission in the Indian Army. In India he saw much hard service: was present at the two sieges of Mooltan, the battle of Gujerat, the relief of Lucknow, under Havelock, and

his fingerless right hand bore testimony to the wounds he received.

But his claim to fame and public notice rests upon his work as an African explorer, at a time when a dark pall of ignorance still spread over most of Central Africa, and when the real sources of the Nile were still a mystery. In 1859 Burton and Speke returned from the heart of Africa, after the former had discovered Lake Tanganyika, and the latter Lake Ukerewe, which he named the Victoria Nyanza, and rightly conjectured to be the main source of the Nile. But the two allies quarrelled, and Grant from the first championed his friend Speke, and accompanied him in 1860 when he was commissioned by the Royal Geographical Society to lead an expedition for the exploration of the Victoria Nyanza. Crossing to the mainland from Zanzibar, the travellers marched by Unyanyembé to the country on the west shore of the lake. There they made friends with the King Rumanika, of whom, and the men who constituted the ruling population of the region, Grant often spoke in kindly remembrance. He had a high opinion both of the country and of the people, and was wont to compare the chiefs, with their retainers, to the old chiefs of his native Highlands, who, like them, were cattle rearers and cattle raiders, proud of their descent, scornful of work, but hospitable and honourable after their own fashion. In July, 1862, the explorers reached their goal, the point where the Nile issues from the northern shore of the Victoria Nyanza, thus verifying Speke's prediction. They followed the Nile for 120 miles, when they were obliged to leave it, but they struck it again 70 miles lower down, and at length reached Gondokoro, in February, 1863, where they met Samuel Baker, who had been sent out to assist them. On their return to England the two explorers were received with enthusiasm. Grant was given the Gold Medal of the Royal Geographical Society in 1864, in which year he published an interesting and instructive work, under the title "A Walk across Africa;" he also contributed to the account of the botany of the expedition, which fills a volume of the 'Transactions of the Linnean Society.' In 1866 he was made a Companion of the Bath. In 1868 he served in the Abyssinian campaign, and for his services was made a Companion of the Star of India. He became a Fellow of the Royal Society in 1873.

He was one of the simplest, most modest, and genial of men, and a universal favorite: a man of commanding stature, but with the kindliest expression of face. After his return from Abyssinia, his time was mostly spent between London and Nairn. His death has caused a sad blank in the large circle of his friends and acquaintances.

J. T. W.

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